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PECULIARITIES OF PERIPHERAL VISION.

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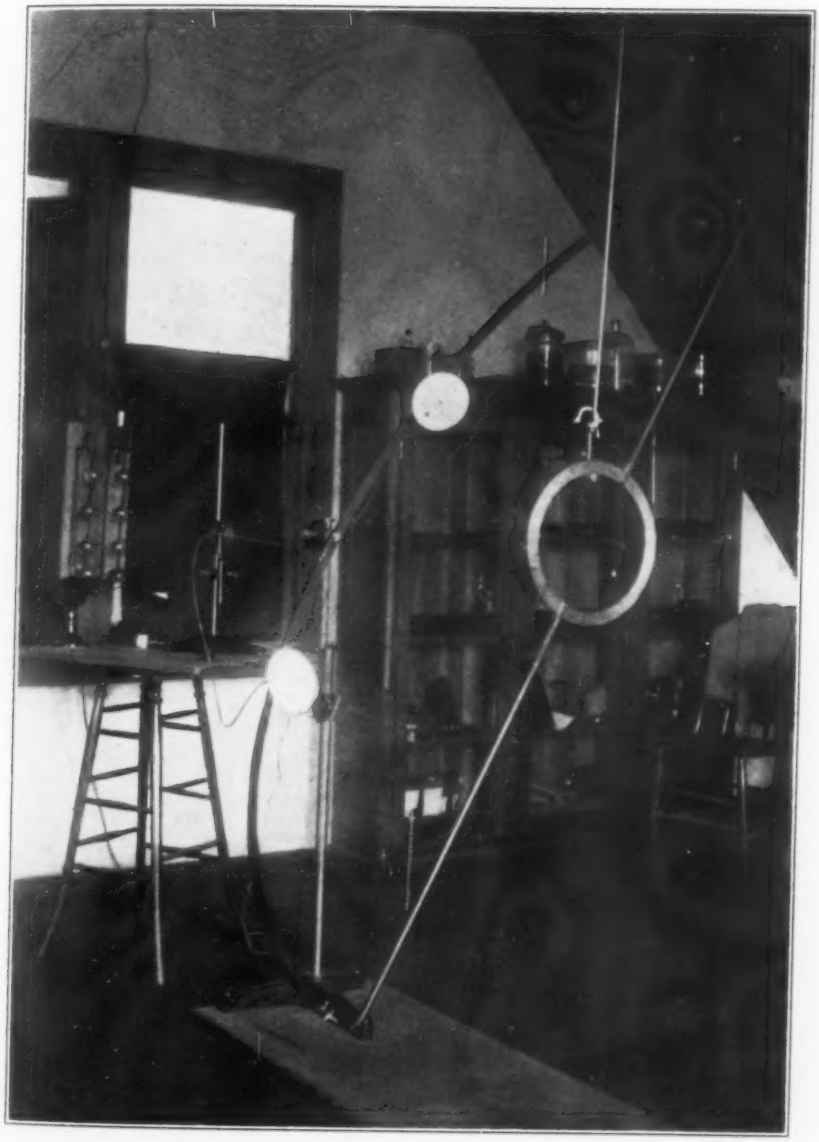
I. Space Values of the Peripheral Retina.

I. Introduction. 1. *Problem.* — The size of objects, as they appear to the eye, varies with the position on the retina of the images of the object. Imaged on the fovea, the object appears of one size; imaged on the periphery of the retina, it appears differently. Analogies to this fact are well known in the perception of extent by the various regions of the skin of the body. But such differences in space perception as exist between the temporal and nasal regions of the retinæ of the two eyes, are not paralleled by any similar facts in the province of tactual space perception. That these differences which are striking and interesting so easily escape attention, is to be attributed, in part, to the dominance of foveal vision, and, in part, to the absence of objects in the peripheral field, between which comparisons could be made. If, however, two discs of white cardboard, each provided with a black spot on its periphery, be rotated in the field of vision of one eye, in such a manner that the images of the disks fall on widely different portions of the retina, the differences in the size of the orbits described by the two spots, instantly become apparent. Furthermore, if the black spots are attached to movable radii of the discs, so that the area included in the orbit of each spot can be increased or diminished by lengthening or shortening the radius, a convenient measure exists for determining the amount of spatial disparity between one part of the retina and another. Under these conditions, not only differences in size appear, but

changes as well in *rate* of speed of the moving spot and *form* of orbit. While these differences of size, rate of motion and form, which exist between the fovea and the periphery of the retina, have all been previously observed, no exact and exhaustive study has been made of them. Exner [1] first noticed that a moving object appears to move more rapidly when its image falls on the periphery of the retina than when it is seen directly. Both Helmholtz [2] and James [3] have pointed out the disparity between the peripheral and foveal perception of size. The illusion of form in indirect vision has been described by Helmholtz [4] and others.[5] The purpose of this paper is to describe the results of observations on the perception of size in various regions of the retina. It is the first of a series of three papers which have as their general theme the peculiarities of peripheral vision. The second and third papers will deal with the illusion of form and the perception of motion in indirect vision.

2. *Apparatus.* — As is evident from the photograph which is here reproduced (Plate I.), the apparatus consisted of a large perimeter. The objects, the sizes of which were compared, were two white cardboard discs each bearing upon it a black spot. These discs were fixed upon a spindle which was so mounted in a metal frame that the frame, spindle and disc could be moved along the perimeter and clamped, by means of a set-screw, at any point desired. An essential condition of the experiment is, that both discs move at the same rate of speed. This requirement was met by turning the discs from the axle of the same pulley. The motion of the pulley was transmitted to the spindles by flexible shafts such as are used by dentists in their drilling machines. By this device, identity of rate of speed is undoubtedly secured; but identity of direction of motion is possible only when the discs, so to speak, have their backs to each other. When the shafts are bent in any other than a direction parallel to that of the axle of the pulley, the directions of revolution of the two discs become opposed. Since

[1] *Pflueger's Arch.*, 38, 217.
[2] *Physiologische Optik*, 2d ed., 697.
[3] *Principles of Psychology*, II., 140.
[4] *Loc. cit.*, 697.
[5] Sanford, *Exper. Psych.*, 192.



such a bending of the shafts was necessary with this apparatus, the direction of motion of the spots was opposed, while the rate of motion which was about 9 revolutions in 10 seconds, was the same. That difference of direction of the moving spots exerted no influence on the results of the experiments was shown by another form of apparatus in which identity both of rate and direction of motion was secured. This apparatus was improvised with the Hering indirect color mixer. (The two small wheels upon the table of the machine, which are set at an angle of 45 degrees to the plane of the large wheel, were unscrewed from the table, turned about until their planes coincided with that of the large wheel and, there, clamped. A belt running in the grooves of the three wheels, insures identity of rate and direction of motion. Attached to the spokes of the small wheels were two white discs each bearing a black spot upon its periphery.) The pulley, from the axle of which the flexible shafts were driven, was itself driven by a small electromotor, the speed of which was controlled by a lamp rheostat. Contrary to what was just stated, the cardboard discs were not themselves the objects of comparison, but rather the space marked off by the black spots when the discs were rotated. Each spot was pasted on a strip of cardboard, similar in form to a tennis racquet, which was held to the disc by being inserted under two flat loops of paper. These loops were fixed to the disc, crosswise to its radial direction. Since the handle of the cardboard strip passed tightly under them, the spot could be pushed towards the center of the disc and the radius of its orbit made less by any desired amount. The discs used in these experiments permitted change in one direction only: their orbits could be diminished in amount, not enlarged. The amount of reduction of the orbit was measured by a mm. scale which was pricked upon the strip and disc.

The perimeter was made of channel-iron, bent in semi-circular form, 25 mm. in width. The ends of the semi-circlewere joined by an iron rod, in the middle of which was riveted a flat ring of steel, 250 mm. in diameter. The perimeter is capable of describing, by revolution, a hemisphere, the center of which, is the center of the steel ring and the pole of which, is the middle

point of the semi-circular arc. In making an observation, the observer sat upon a chair with his head so placed inside of the ring, that the observing eye coincided with its center and with the line of sight of the eye directed toward the pole of the hemisphere.

Dimensions of parts of apparatus:

Radius of perimeter	97.0 cm.
Length of flexible shafts	96.0 "
Diameter of discs	14.5 "
Diameter of orbit	8.6 "
Range of orbit	1.5 "
Diameter of black spots	1.4 "

The size of the retinal image of the orbit is given by the proportion

$$977 : 16 :: 86 : x,$$
$$x = 1.4 \text{ mm.}$$

When the radius of the orbit was reduced by 15 mm. the retinal image measured 0.9 mm. The actual areas on the retinæ, that were being compared, varied from 1.4 to 0.9 mm. in diameter.

3. *Method.* — The method of this problem was Fechner's method[1] of equivalents. With the observer's eye in the position just described, the two discs were presented at different points in the field of vision. The difference in size of the two orbits, which is apparent when the images of the discs fall, one upon the superior and the other upon the inferior retina, or one upon the nasal and the other upon the temporal retina, can easily be measured by reducing the orbit of the larger until it equal, subjectively, the orbit of the smaller. It took, usually, six settings of the radius to determine the place of subjective equality. Gradations were at first large, but were reduced, as the place of equality was approached, to changes of 1 mm. The changes were made in one direction only: from larger to equality. The figures given in the tables are averages of the last judgment of larger and the first certain judgment of equality.

Four meridians and three parallels of latitude of the hemispherical field of vision were subjected to exploration. The

[1] *Elemente der Psychophysik*, 131 f.

meridians were horizontal, vertical and two oblique (45°). The parallels of latitude were ten, twenty and twenty-five degrees from the pole of the visual hemisphere. (See Fig. 1 for a representation of the field.) The fact that observation with any degree of accuracy, in the extreme periphery of the field of vision, becomes very difficult, restricted the work to this range.

The observations are of two sorts, according as comparisons were made (*a*) between the two peripheral portions, or (*b*) between fovea and periphery, of the retina. The first class of experiments will be referred to as *peripheral*, the second class as *foveal-peripheral*, comparisons. In the peripheral comparisons, the discs were situated on the same meridian, equidistant from, though on opposite sides of, the pole. In the foveal-peripheral comparisons, one disc was situated at the pole, the other in the periphery upon some one of the four meridians. Since there were four meridians and three parallels in our field of vision, there would be, for the peripheral comparisons, twelve positions of the discs and, for the foveal-peripheral comparisons, twenty-four positions. Observations were made with one eye at a time.

II. Results. — The results of the experiments, owing either to differences of conditions under which the observations were made or to the carrying out of some subsidiary purpose, fall into three classes. (1) Quantitative measurements in the manner described, of the illusion in different parts of the retina. Complete sets of quantitative observations for the positions described in 1.3, were obtained from two observers, Drs. Magnusson and Frye. All observations were made during the college year, 1906–1907. The majority of measurements were made in the tenth, eleventh and twelfth months of 1906. Repetitions of over one half of the observations, for the purpose of determining their constancy and accuracy, were carried on during the third, fourth and sixth months of 1907. (2) Qualitative experiments for the same set of positions as the preceding, made upon two observers, Miss Waddingham, a student in the University, and the author. In these observations, no attempt was made to determine the amount of the illusion. The observer indicated the disc that appeared larger. (3) Crude qualitative observations on the horizontal and vertical meridians, for one

parallel of latitude, usually twenty-five degrees, made upon many persons, students and others, who chanced to come to the laboratory. Also, experiments in which the perimeter was not used. In these experiments, the materials were discs of cardboard or of clay or, infrequently, coins placed upon a table in front of the observer. These observations were made with view to seeing whether there was any connection between the illusion and left-handedness.

1. *Quantitative Experiments.* — (*a*) Peripheral comparisons. The results of the experiments may be made clear by imagining the perimeter in the vertical meridian with the discs 25° north and south of the fixation point. Furthermore, assume that the perimeter takes up, successively, the positions, northeast-southwest, east-west, southeast-northwest. In the vertical meridian,

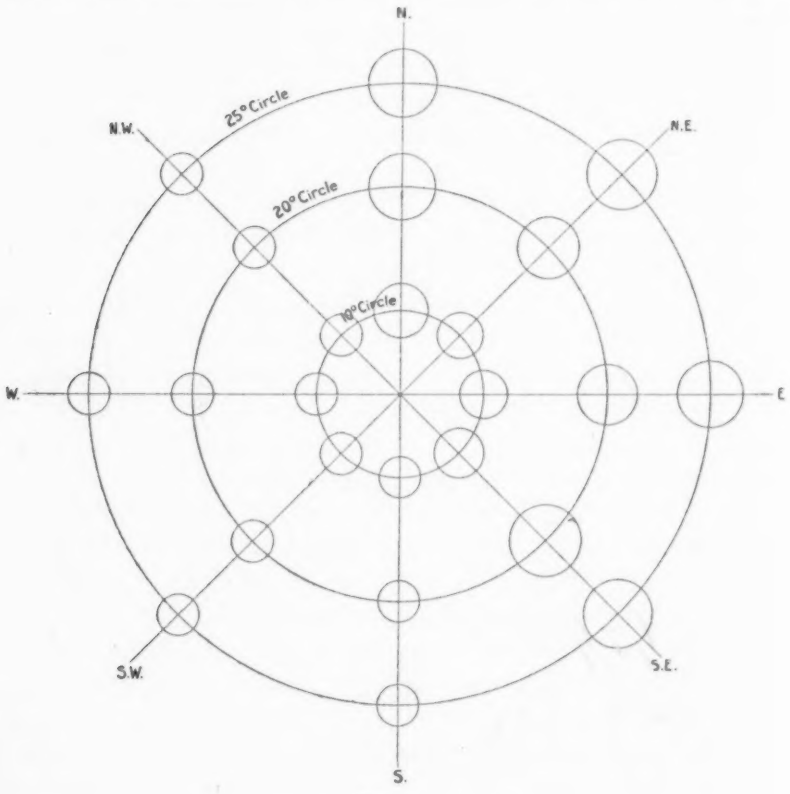


FIG. 1.

the upper disc appears decidedly larger to *both* eyes. In the oblique position, the right disc appears larger to *both* eyes. In the horizontal meridian, the right disc appears larger to *both* eyes. In the oblique meridian, the right disc appears larger to *both* eyes. A similar result is obtained when the discs are set at 10° and 20° from the center. Stated generally, the result is that objects in the right half of the field of vision appear larger than exactly similar objects occupying symmetrical positions, in the left half of the field of vision. This result is, with some exceptions, universal for right-handed observers. The number of exceptions will be stated, when the third class of experiments comes to be considered. The result so far attained, that the right half of the field of vision is constantly enlarged over the left half, is expressed graphically, in Figs. 1 and 2. Without

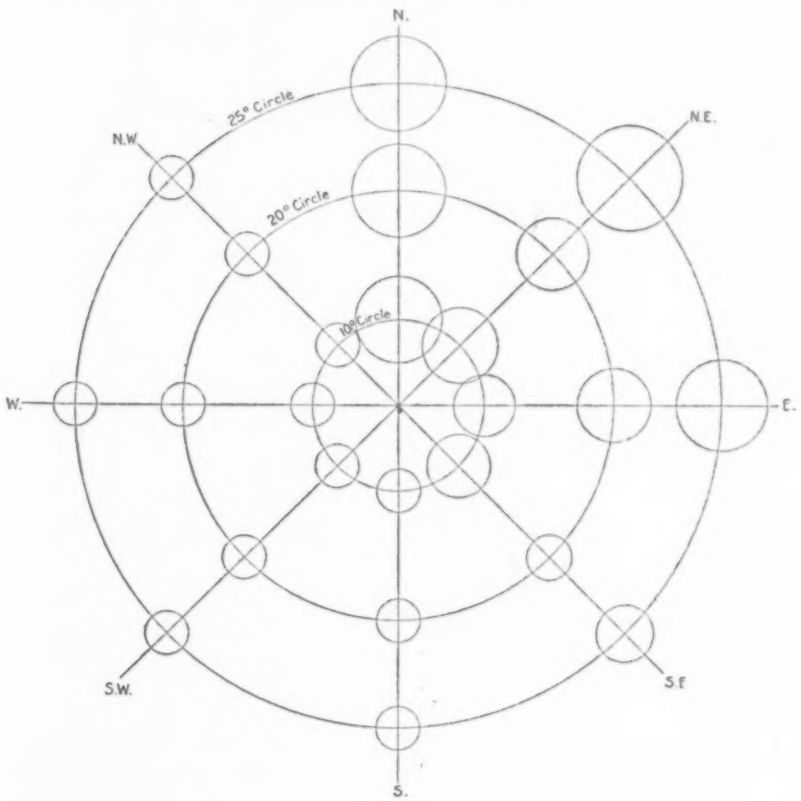


FIG. 2.

attempting to be a plane projection of, the figures represent, the hemispherical field of vision of the right or left eye. The horizontal, vertical and oblique lines stand for the four meridians; the three concentric circles represent the three parallels of latitude. The smaller circles which are placed at the intersection of the meridians and parallels, represent both the positions of the discs, when comparisons were made, and the amount of enlargement a disc in any position undergoes. The circles in the left half of the field are made with a standard radius of 10 mm.[1] The radii of the circles in the right half of the field are *increased* by as many mm. as was found necessary to decrease the radius of the actual disc to make it appear equal to the other. The amounts in mm. by which the radius of any disc was reduced before it appeared to the other, are given for each eye separately, for observers M. and F. in Tables I. and II. The numbers in the tables mean that, to the right eye, Re, and to the left eye, Le, the north, northeast, east and southeast discs appeared larger than the corresponding south, southwest, west

TABLE I.

(MAGNUSSON.)

	25°	20°	10°
N. and S.	Re 7 Le 7½	Re 6 Le 6	Re 2½ Le 3½
	Re 6 Le 6	Re 5½ Le 6½	Re 3 Le 3
N.E. and S.W.	Re 7½ Le 7½	Re 5 Le 5	Re 0 Le 1
	Re 7 Le 6½		
E. and W.	Re 5 Le 9	Re 5 Le 4½	Re 2 Le 1
	Re 7½ Le 2½	Re 4½	
	Re 7½	Le 4	
S.E. and N.W.	Re 7½ Le 7½	Re 7½ Le 7½	Re 2 Le 2
	Re 7 Le 5		Re 1½ Le 2½

[1] In the reproductions, the drawings have been reduced proportionally approximately two sevenths.

TABLE II.

(FRYE.)

	25°	20°	10°
N. and S.	Re 9½ Le 11½	Re 10½ Le 10½	Re 8½ Le 10
	Re 11 Le 15	Re 12½ Le 12½	Re 11 Le 10
N.E. and S.W.	Re 12½ Le 14½	Re 2½ Le 7½	Re 6½ Le 8½
	Re 15 Le 15	Re 7½ Le 9½	Re 8
E. and W.	Re 11½ Le 11½	Re 10½ Le 6½	Re 8 Le 8
	Re 11 Le 9	Re 2½ Le 5 Re 12	Re 0 Le 0
S.E. and N.W.	Re 0 Le ½	Re 0 Le ½	Re 5 Le 4
	Re 0 Le 2½ Re 2½		

and northwest discs, by as many mm. as are indicated by the figures. The tables also show the number of times the observations were repeated. For each position, the table has two spaces, an upper and a lower space. Where both are filled, the observation was made twice. In some cases, the same observation was made three times; all measurements are given in the table. To obtain the measurements from which Figs. 1 and 2 were made, all right eye measurements, for any position, were averaged together and all left eye measurements, for the same position, were averaged together. The average of these averages was used in the drawing. Since, as the tables show, there is no serious discrepancy between the readings of the two eyes, the averaging of the figures can not conceal any essential result. In addition to the result already mentioned, the figures and tables show that the enlargement is greatest in the vertical meridian and least in the southeast-northwest meridian; that it is greater at 25 than at 10 and that the amount of enlargement varies with the observer.

If it is true that objects in the right half of the field of vision are uniformly enlarged over objects in the left half, it follows

as a corollary, that there must be a line of transition between the two halves where similar objects appear of equal size. Experiment verifies the inference. The discs were set, in turn, at 10°, 20° and 25° and the perimeter placed, as an initial position, in the vertical meridian. A glance at Figs. 1 and 2 will show that the upper disc is, here, largely overestimated; while in the northwest meridian, the upper disc is smaller. Between these two points, one would think, the line of transition must run. But experiment does not verify this supposition. It turns out that, when the observer moves the perimeter from the vertical meridian towards the west, until the discs appear of equal size, the perimeter occupies a position between the northwest and the horizontal meridians. The measurements that are about to be given, were made by each observer, with each eye separately, by moving the perimeter from the horizontal meridian (ascending) towards the vertical, until the discs appeared of equal size, and from the vertical meridian towards the horizontal (descending), until the discs appeared equal in size.

			Observer M.	Observer F.
10 degrees	Right eye	Ascending	16.20°	25.92°
		Descending	16.92	22.32
	Left eye	Ascending	28.08	9.72
		Descending	34.20	9.00
20 degrees	Right eye	Ascending	18.00	20.52
		Descending	25.92	21.24
	Left eye	Ascending	18.00	9.72
		Descending	20.88	9.00
25 degrees	Right eye	Ascending	7.20	7.20
		Descending	14.40	39.60
	Left eye	Ascending	19.08	5.40
		Descending	25.56	32.40

The measurements vary greatly. This irregularity may be attributable to the circumstance that the observations were made at the end of the college session, when both observers were pressed with other duties. The other characteristic of the readings, that the line of transition leans much more toward the horizontal than would have been expected from the quantitative observations, the author is at loss to explain.

(*b*) Foveal-peripheral comparisons. In these observations, as has been stated, one disc was placed at the center of the field of vision; the other was placed upon some one of the four meridians, 10°, 20° or 25° from the center. The results, in mm. for observers M. and F., are given in Tables III. and IV. Observations with the right eye (Re) and left eye (Le) are recorded separately. The tables also show the number of repetitions. Sixteen out of twenty-four of M.'s and thirteen out of

TABLE III.

(M.)

	25°	20°	10°
N.	Re 7½ Le 8½	Re 4½ Le 5½	Re 3½ Le 4½
	Re 5 Le 7½		Re 0
N.E.	Re 7 Le 8	Re 5½ Le 5½	Re 4 Le 3
		Re 5 Le 6	
E.	Re 6½ Le 6	Re 3½ Le 3½	Re 4 Le 3
	Re 8½ Le 8½	Le 4½	Re 1½ Le 2½
S.E.	Re 6½ Le 6	Re 5½ Le 6	Re 4½ Le 4
		Re 3 Le 5	
S.	Re 5 Le 4	Re 3½ Le 1½	Re 4 Le 5
	Re 0 Le 2½	Re 0 Le 0	Re 2½ Le 2½
S.W.	Re 4 Le 5	Re 4 Le 3	Re 3½ Le 3½
		Re 4½ Le 4½	
W.	Re 3½ Le 2½	Re 1½ Le 2½	Re 3½ Le 3½
	Re 2 Le 0	Re 1½	Re 4
N.W.	Re 4½ Le 4½	Re 3½ Le 4½	Re 3½ Le 1½
		Re 3 Le 4	Re 4

TABLE IV.

(F.)

	25°	20°	10°
N.	Re 10 Le 8½	Re 5 Le 7½	Re 6 Le 6
N.E.	Re 7½ Le 10	Re 5½ Le 3½	Re 4 Le 3
E.	Re 8½ Le 6½	Re 5 Le 0 Re 0 Le 2½ Le 0	Re 5 Le 3
S.E.	Re 6½ Le 8	Re *2* Le 0 Re 0 Le 0	Re *5* Le *2*
S.	Re *1* Le *4* Re *2* Le ½	Re *7½* Le *7½* Re *7* Le *9*	Re 0 Le *3* Re 0 Le *3½*
S.W.	Re *4* Le *4* Re *4* Le *5*	Re *5* Le *5*	Re *4½* Le *5½*
W.	Re *7* Le *3* Re *4* Le *5*	Re 0 Le 0 Re 0 Le 0	Re 1 Re 0 Le 0
N.W.	Re *4* Le *4* Re *3½* Le 0	Re 0 Le *1*	Re ½ Le 0 Re 2 Le 2½

twenty-four of F.'s observations were repeated. The non-italicized figures of the tables signify that the peripheral disc appeared larger than the central disc by the number of mm. indicated by the figures. The italicized figures signify that the central disc appeared larger than the peripheral disc by the number of mm. indicated by the figures. The measurements of these tables, averaged as were those of Tables I. and II., are

expressed graphically in Figs. 3 and 4. The field of vision is represented here, as in Figs. 1 and 2. The small circle at the center of the field stands for the central disc; the other circles in the periphery of the field, express by their diameters, the exact amounts by which a disc at any place in the periphery, appeared larger or smaller than the central disc. In those positions in which the central disc was larger than the peripheral disc, the central disc was reduced until it equalled the

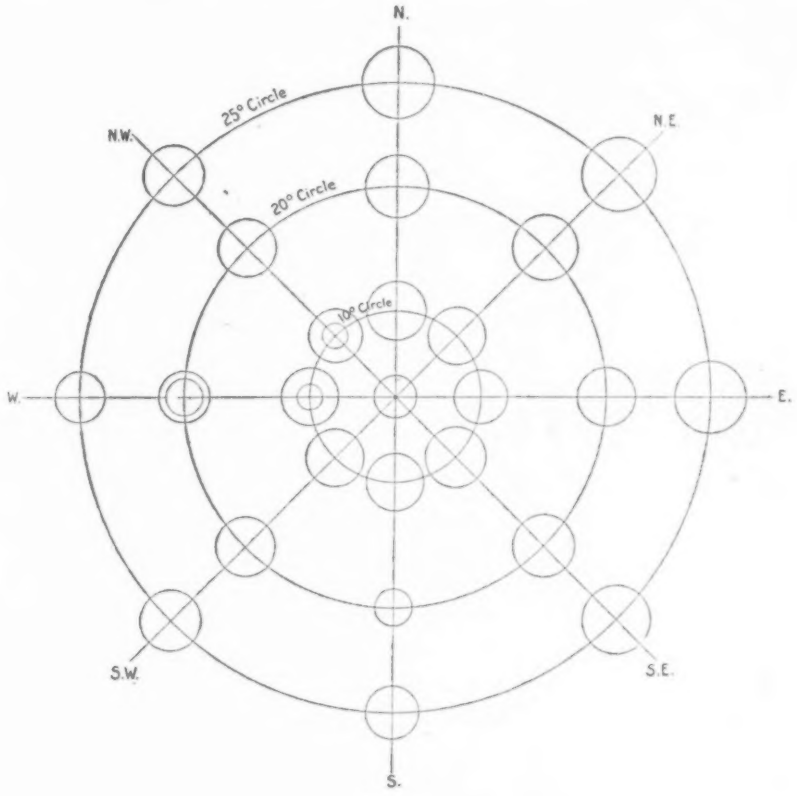


FIG. 3.

peripheral disc. In the drawing, the peripheral circle is diminished by as many mm. as the central disc was diminished. In M.'s observations, three contradictory reports were given. In the figures, both results are expressed by the two concentric circles.

As is evident from the figures, there is a very decided enlargement, in the results of both observers, of the right-upper half of the visual field over the foveal field and an equally

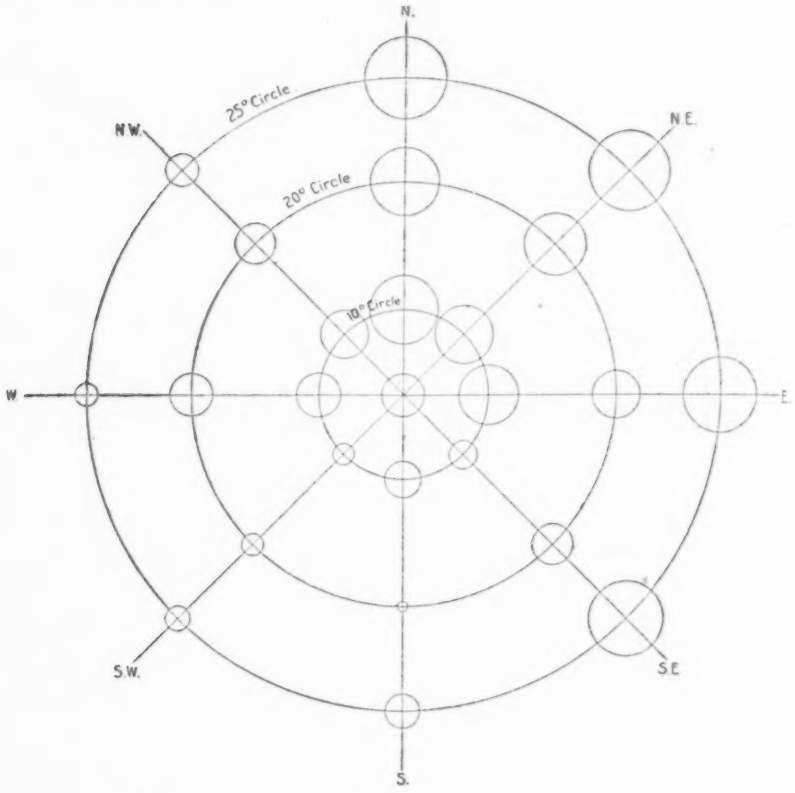


FIG. 4.

noticeable tendency to underestimation (in the case of F.) or to slight overestimation (in the case of M.) of the left-lower half of the visual field over the foveal field. So far, then, from its being true, as James[1] asserts for the retina, from analogy to Fechner's experiments on the skin, that the fovea, as the point of keenest sensibility, imposes its standards of space measurement upon the peripheral parts of the retina, it appears that those parts have a well marked spatial individuality, which resist, surprisingly, reduction to a common unit.

[1] *Prin. of Psych.*, II., 214 and 178.

2. *Qualitative Experiments.* — (*a*) Peripheral comparisons. These observations were made under the same conditions as those already recorded. As no measurements were taken, it will not be possible to exhibit the results in tables and figures. From the verbal responses of the observers, however, the nature of the result was very easily gathered. Both observers, W. and S., confirm the result found for M. and F., that the right half

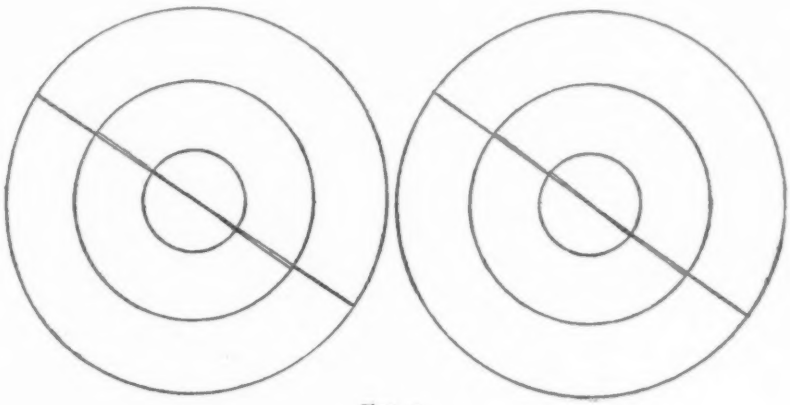


FIG. 5.

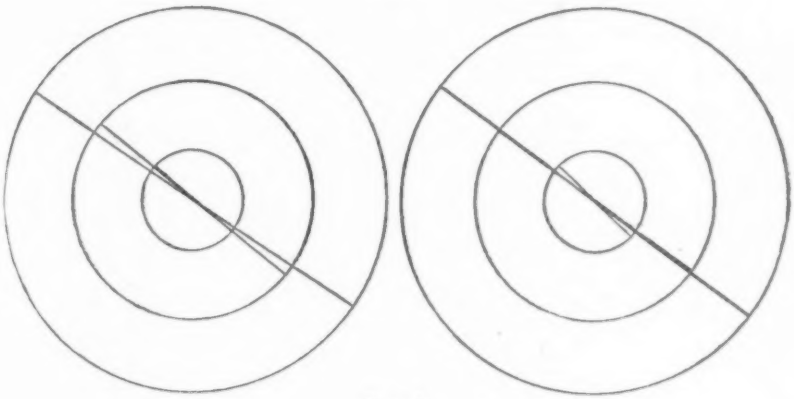


FIG. 6.

of the field of vision is constantly enlarged over the left half of the field. In fact, if the precise amount of magnification is left out of account, Figs. 1 and 2 may serve to represent the results of the experiments for W. and S.

The line of transition between the two halves of the field, was determined for W. and S. in the same manner as for M. and F. The measurements are given both in degrees and, expressed graphically, in Figs. 5 and 6. In the figures, the ascending and descending values of each eye are averaged together. The three circles stand for the three parallels of latitude, 10, 20 and 25. The results for each eye are expressed separately.

			Observer W.	Observer S.
10 degrees	Right eye	Ascending	25.56	50.40
		Descending	49.32	36.00
	Left eye	Ascending	23.04	46.08
		Descending	43.20	32.40
20 degrees	Right eye	Ascending	28.80	32.40
		Descending	42.48	39.60
	Left eye	Ascending	28.08	30.60
		Descending	39.96	46.44
25 degrees	Right eye	Ascending	32.40	37.44
		Descending	36.72	36.36
	Left eye	Ascending	28.80	36.36
		Descending	36.36	30.60

The measurements show much more uniformity than did those of M. and F. The slant of the line of transition is, however, less than 45°.

(*b*) Foveal-peripheral comparisons. The results of the observations of W. and S. agree with those of M. and F., with the exception that the left half of the field is always, though slightly, larger than the foveal field. The right-upper field is very decidedly larger than the foveal field.

(*c*) Retinal regions and cerebral connections. So far, the description of these phenomena has gone on in terms of the visual field. In the peripheral comparisons, the orbit[1] of the disc in the right half, appears larger than the orbit of a similar disc in the left half, of the monocular field of vision. In the foveal-peripheral comparisons, the orbit of the disc in the right-

[1] The author has spoken all along of the disc undergoing enlargement and of its being reduced, when, speaking more strictly, he should have said 'orbit' of the disc.

upper half, appears larger than the orbit of a similar disc in the foveal field of vision. We must now see upon what parts of the retina fall the images of those discs whose orbits in the field of vision appear so much enlarged. (i) Peripheral comparisons: Imagine the perimeter in the horizontal meridian with the discs 25° to the right and left of the fixation point. Let the observation be supposed to be made first with right eye. Under these conditions, with the observing eye at the center of the hemispherical field, the pole of the field is imaged upon the fovea, the image of the perimeter coincides with the retinal horizon and the right and left discs are imaged, respectively, 25° nasalwards and temporalwards of the fovea. Without changing the position of the perimeter or of the discs, let the left eye be placed at the center of the field. Again, the image of the pole falls upon the fovea and the image of the perimeter coincides with the retinal horizon, but the images of the right and left discs now fall 25° temporalwards and nasalwards, respectively, of the fovea. That is to say, the images of the discs fall upon corresponding points of the retinæ. The same result obtains, if the perimeter be imagined to take up its position in the vertical and two oblique meridians. The images of the discs fall always upon corresponding points of the binocular visual apparatus. As is well known, the left half of this apparatus, the nasal half of the right, and the temporal half of the left retina is connected with the left occipital hemisphere. The constant enlargement, therefore, of objects in the right half of the peripheral field of vision, has its anatomical ground in the fact that the images of objects so situated, fall upon the left half of the cyclopean retina and that this part of the retina is connected with the left hemisphere of the cerebrum. (The writer disregards, in the absence of evidence, the possibility that the anatomical ground of the results is the number of retinal elements in the corresponding portions of the retinæ.) The relations between the field of the vision, corresponding points of the retinæ and the visual cortex are well represented by Fig. 7 which, with the addition of the visual field is practically, a reproduction of Fig. 347, vol. 2, of Schaefer's *Text-book of Physiology*. The correspondence between the two retinæ is shown in the figure by representing the

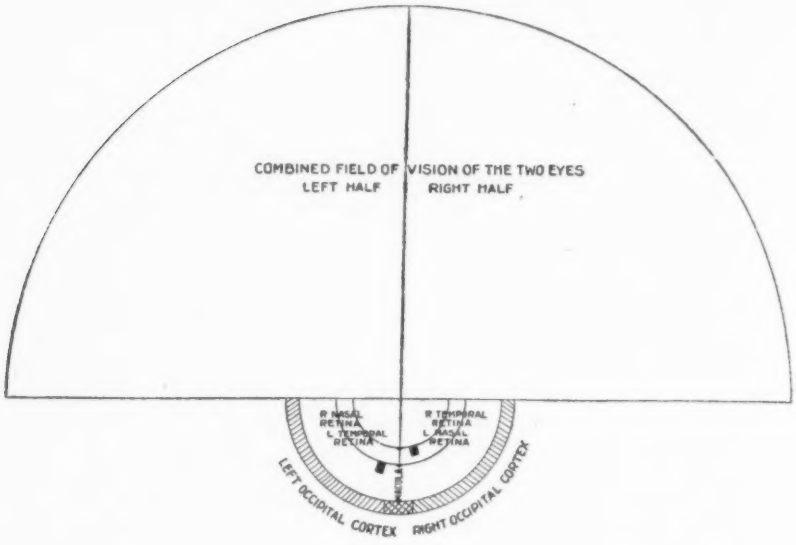


FIG. 7.

left retina as having been set inside of the right; there is a point for point correspondence between the two surfaces. Besides showing the connection between the two halves of the visual cortex and the two corresponding halves of the retinæ, the diagram also shows, by the doubly shaded portion of the part representing the cortex, that the macular region of the retina is innervated from both hemispheres. (ii) Foveal-peripheral comparisons: The retinal and brain conditions of these comparisons are exhibited in the same figure. The anatomical conditions of these observations differ from those of the other set in that the fovea, upon which was formed the image of the central disc, is connected with both hemispheres. In view of the fact that the retinal image of the orbit of the spot was 1.4 mm., while according to Koelliker's measurements,[1] the fovea is 0.18 to 0.225 mm., in diameter, it would be more exact to speak of these observations as macular-peripheral comparisons. As cited by Helmholtz, Krause and Koelliker give the horizontal diameter of the macula as 2.25 and 3.24 mm., respectively. According to these measurements, the image of our disc would easily fall upon the macula.

[1] Cited by Helmholtz, *Physiol. Optik*, 2d ed., 37.

When this relation between the enlargement of objects in the visual field and the cortex was pointed out to them, it was suggested by my colleagues, Dr. Magnusson and Dr. Savery that a reversal of the illusion might be obtained with left-handed observers who are, presumably, right-hemisphered. The third class of experiments is concerned with that point.

3. *Crude Qualitative Experiments.* — For the most part, these experiments which were all comparisons of the peripheral sort, were made with clay discs (poker chips) 38 mm. in diameter. The discs were placed upon a black cloth at a distance of about 60 mm. from a white fixation mark which was equidistant from the inner edges of the two discs. The cloth with the discs upon it, was placed on a table. The observer standing alongside of the table looked, with one eye at a time, at the white mark between the discs. With his eye in this position, the observer noticed which disc appeared larger. The distance of the eye of the experimenter from the fixation point was about 75 cm., although, undoubtedly, there was much variation on both sides of this distance. Taking this as the average distance, the retinal images of the discs would fall 4°32′ on either side of the fovea. A much smaller number of observations were made with cardboard discs 14 cm. in diameter, placed upon black velvet, with their inner edges 60 mm. from the fixation point. With these materials, the difference in size is much more apparent than with the smaller discs. Some observations with the perimeter are included in this class. Finally, about fifteen observations which were made by correspondents with the help of a questionary blank upon which was printed two white discs 51 mm. in diameter, in a field of black. The inner edges of the discs are 37.5 mm. from the fixation point. The observer is directed to hold his eye two and one half feet (75 cm.) from the paper. At this distance, the images of the discs would fall 2°50′ from the fovea. The ages of the observers varied from twelve to sixty years. It is evident that the conditions under which these observations were made were neither uniform nor exact. Yet, in spite of these faults, a fairly definite result does emerge from the observations as a whole. In all, 183 tests were made. According to the answer to the question, which

disc (right or left) appears larger, the experiments fall into six classes. Observations were made both in the horizontal and vertical meridians. In the numerical statement of the results about to be given, only the results of the observations in the horizontal meridian are taken account of, as having bearing on the point at issue. It may be said that, in the vertical meridian, at least 90 per cent. of the results show that the upper disc appeared larger to both eyes. (1) To one hundred persons, the right disc appeared larger to both eyes. (2) To forty-five persons, the left disc appeared larger to both eyes. (3) In sixteen cases, the right disc appeared larger to the right eye and the left disc larger to the left eye. (4) In thirteen cases, the left disc appeared larger to the right eye and the right disc larger to the left eye. (5) In five cases, the discs appeared unchanged in size to the right eye, but different in size to the left eye. Of these cases, the right disc seemed larger four times and the left disc larger once. (6) In four cases, the discs appeared unchanged in size to the left eye, but different in size to the right eye. Of these cases, the right disc appeared larger twice and the left disc larger twice. In making this classification of the experiments, no attention was paid to right- or left-handedness. With the observations so distributed, each class was further divided with respect to right-handed, ambidextrous and left-handed individuals. Of the first class, seventy-six were right-handed, eight ambidextrous, sixteen left-handed. Of the second class, fifteen were right-handed, three ambidextrous and twenty-seven left-handed. Of the third class, nine were right-handed, six ambidextrous, one left-handed. Of the fourth class, three were right-handed, two ambidextrous, eight left-handed. Of the fifth class, one was right-handed, none ambidextrous, four left-handed. Of the sixth class, one was right-handed, one ambidextrous, two left-handed. The results of classes one and two, as unequivocal, are expressed graphically in Fig. 8. The results certainly indicate a tendency to a relationship between right-handedness and the enlargement of the right disc and left-handedness and enlargement of the left disc. Seventy-six per cent. of the observers to whom the right disc looked larger to both eyes were right-handed. Sixty per

cent. of the observers to whom the left disc appeared larger were left-handed. Of the forty-three cases of left-handedness tested, twenty-seven find the left disc larger while sixteen find the right disc larger. It is true that this is not an over-

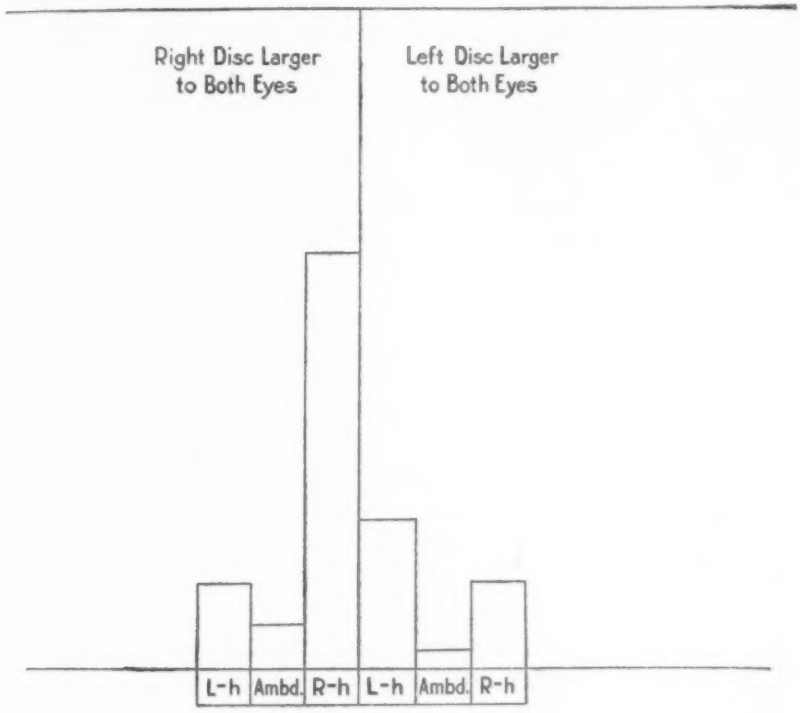


FIG. 8.

whelming majority, but there is an excess of five and one half cases, or 12.8 per cent. of the total number observed, which would be difficult to account for, if chance alone were operative. A serious error of which the author was not appreciative until too late, which may have obstructed a more decided result, is the fact that in all experiments except those carried out with the perimeter, the images of the discs fell at a distance not more than 5° from the fovea. Reference to Tables I. and II. will show that the amount of enlargement at 10° is comparatively slight. Furthermore, as is well known the macular region of the retinæ is innervated from both hemi-

spheres. Unless the images of the discs fell upon portions of the peripheral retina that are connected with one hemisphere or the other, not with both, the experiment would not signify. To determine the angular distance from the fovea to which double innervation extends, appeal was made to cases of cortical hemianopsia. A case of complete, right, homonymous hemianopsia described by Foerster, is cited by Wilbrand.[1] In this case, the line of division between the blind and normal fields of vision was vertical, slightly to the right of the vertical meridian. In the region of the fixation point, on the horizontal meridian, the line passed two degrees outside of the fixation point. If this case may be taken as typical, most of the experiments with the discs, must have fallen on truly peripheral portions of the retinæ. The results of this class of experiments are certainly not final, but taken along with the outcome of the other experiments, that the right peripheral field is constantly enlarged over the left peripheral and the foveal fields, they seem to warrant a suggestion of a theory of right-handedness.

All theories that make right-handedness a matter of use and wont, mere convention, seem to be denied serious consideration, by the fact that it is hereditary. The consensus of opinion, supported by some comparative measurements[2] of the weights of the two hemispheres, advocates the view that the left hemisphere of the brain is the ground of the phenomenon. But aside from this general reposing of the question in the ascendency of the left hemisphere over the right, there has been no precise formulation of the manner in which this ascendency could make individuals right-handed. It has not been asserted whether the ascendency of the hemisphere resides in the motor or in the sensory region. A consideration will show that the initial difference must rest in the sensory aud not in the motor areas of the cortex. The consideration is, that the current of innervation is always from the sensory to the motor region, the current flows only in the 'forward direction.'[3] The contraction of muscles of the right side of the body is only a consequence

[1] *System of Diseases of the Eye*, Norris and Oliver, Vol. II., 305.
[2] Dr. Boyd's and others cited by Wilson, *Left-handedness*, 188.
[3] James, *Prin.*, II., 581.

of the discharge of nerve cells in the left motor cortex; but, the discharge of these cells is, again, a consequence only, of the discharge of sensory cells centrally or peripherally situated. If this view is correct, motor bilateral asymmetry is merely symptomatic of sensory bilateral asymmetry. Our experiments have shown a very marked difference in sensory function, to exist in the field of vision, the sensations of which, it is hardly necessary to point out, play an important part in animal behavior. Such a difference in sensation would, probably, first bring about reflex movements of the eyes such that the object on the periphery would be plainly seen. Movements of the eyes would be followed by movements of the head; and movements of the head, by movements of the hand. This view is supported by the fact that the time (7 months) at which a decided preference for the right hand had developed in Baldwin's child[1] comes a little later than the time (5 months) at which Raehlmann[2] found that an object on the periphery of the retina was recognized by a child.

III. Discussion of Results. — The manner in which the observations were made, the results of the observations and the theoretical bearing of the results have now been duly set forth. It remains to forestall two possible objections. (1) It may be said the results are due to indistinctness of peripheral vision. It is true that some observers, on some occasions, in certain positions, mentioned that one disc was plainer than the other. For example, in the vertical meridian, F. found the upper disc more distinct than the lower. But, it was only exceptionally that a difference was spoken of. In answer to the objection, then, it may be said (*a*) that there was correlation between the illusion of size and distinctness and indistinctness; (*b*) that there is no reason why indistinctness should be associated with largeness rather than smallness. As evidence of a contrary opinion, it may be pointed out that Helmholtz[3] lays it down as a general principle that plainly perceived differences appear larger than differences of equal magnitude which are not plainly perceived.

[1] *Mental Development, Methods and Processes*, 64.

[2] Cited by E. A. Schaefer, art., 'The Cerebral Cortex,' *Text-Book of Physiology*, vol. ii., 759, without reference to the original.

[3] *Physiol. Optik*, 2d ed., 705.

(2) It may be alleged, in explanation of the facts, that they are due to defective accommodation of the lens, for the periphery of the retina. According to Helmholtz,[1] the retinal image is well defined only in the neighborhood of the axis of the eye. As he gives no measurements or other indications of the amount of decrease in definiteness, for the various regions of the periphery of the retina, it is not possible to say whether these results vary with it. However, as having some value as evidence against this interpretation of our results, it may be pointed out that in the schematic drawing of a horizontal section through a right eye, Helmholtz[2] makes the retina equidistant from the nodal point of the lens, 20° temporalwards and 25° nasalwards, of the fovea. The only experimental evidence with which the author is acquainted, that is pertinent to this point, is the observation made by Aubert,[3] on the eye of a rabbit, of the retinal images in the periphery of the retina. Aubert wished to meet the objection that the results of his experiments on the capacity to discriminate two points on the periphery of the retina, were due to lack of accommodation. The eye-ball of a rabbit was removed, cleaned of fat and connective tissue and mounted in a screen. At a distance of 20 cm., the images of the cardboard discs were plainly visible to the naked eye, formed upon the periphery of the retina, when seen from behind through the choroid and schlera. At a distance of 70 cm., images of the same objects were visible on the periphery, when magnified 30 times by a microscope. Of course, the objection to this method of deciding the matter is, that what holds for the eye of a rabbit need not hold, necessarily, for the eye of man. There are, however, two replies which seem to the author adequate to the objection. (*a*) If the results were due to defective accommodation for the peripheral parts of the retina, one would not expect to find the defect distributed with so much uniformity from right to left and as agreeing so well among four observers. (*b*) Observations show that the same results obtain for 5° from the fixation point where, it must be supposed, the lack of accom-

[1] *Physiol. Optik*, 2d ed., 87.
[2] *Loc. cit.*, 90.
[3] *Physiologie der Netzhaut*, 250.

modation is very slight, if at all existent, as obtain for positions farther out in the peripheral field. (3) The illusion is not due to defective eye-sight. The eye-sight of observers F., W. and S. is normal or better. M. has a very slight scar on the cornea of his right eye. He does not wear glasses. In the crude qualitative experiments, persons who wore glasses kept them on during the observation.[1]

[1] The MS. of this article was received August 20, 1907. — ED.

VOCABULARY AND WORD-BUILDING TESTS.

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At the meeting of the American Psychological Association in December, 1905, Professor Kirkpatrick gave a brief account of a simple vocabulary test, which he has since described more fully.[1] This test I have applied with some modifications to two classes of sophomores and juniors at Cornell University; the discussion of the results of this test forms the first section of this paper. In the second section there is described another test, that of word-building, which exhibits certain points of similarity to the vocabulary test. Attention is also paid to the correlation between these two tests, and between them and school standing.

A. Kirkpatrick's Vocabulary Test.

This test consists, in principle, in the examination of a limited number of words taken by chance from the pages of a dictionary, and the determination of the percentage of these words whose meaning is known, unknown, or doubtful. On the basis of this examination, the 'understanding vocabulary' is estimated at that percentage of the total number of words in the language that the test has indicated. Naturally, the longer the list examined, the more accurate is the index secured. Kirkpatrick is, however, convinced that 100 words form a sufficiently lengthy list upon which to compute the index in question, at any rate, sufficiently well to be representative of one's vocabulary for comparative purposes.

Purpose of the Test. — My object in using the test was: (*a*) Primarily to examine its reliability, *i. e.*, to see whether college students would be apt to overestimate or underestimate

[1] *Pop. Sci. Mo.*, LXX., February, 1907, 157–164. The examination of the test with which this paper deals was nearly completed when the originator's paper appeared, and although he has anticipated therein certain points of criticism, I have published my results in their first form. The reader may also consult E. H. Babbitt's article in the same volume, p. 378.

their vocabulary in the absence of a strict definition-check; (*b*) to determine the average vocabulary of college students; (*c*) to determine the correlation of the corrected vocabulary with ability to construct words in the word-building test; (*d*) the correlation with standing in college classes, and (*e*) the correlation with sex.

Nature and Method of the Test. — (1) The following list of words, the same used by Kirkpatrick, was distributed in the class, with these instructions: Place a plus sign (+) before all the words you know, a minus sign (−) before those you do not know, and a question mark (?) before those of whose meaning you are in doubt. When through, count, and fill out these blanks. Number known . . . Number unknown . . . Number doubtful . . .[1]

abductor	escheat	paid
abeam	escort	pail
abed	eschalot	publication
abet	gourd	pudding
baron	gout	puddle
baroscope	govern	pudgy
barouche	gown	scroll
barque	intercede	scrub
channel	interdict	scruff
chant	interest	scrunch
chanticleer	interim	subcutaneous
chaos	matting	sub-let
decemvirate	mattock	subdue
decency	mattress	tycoon
decide	maturate	tymbal
deception	page	type
eschar	pagoda	
amalgamation	filing	photo-lithograph
amanuensis	fill	rejoice
amaranth	hodman	rejoin
bottle-holder	hoe	rejoinder
bottom	hoecake	rejuvenate
bottomry	hog	skylight
boudoir	lanuginose	skyrocket

[1] The request to write the opposite of a series of ten words, to define ten words, five of which were in the list, and to indicate the magazines and books read within a specified time, which formed a part of the Kirkpatrick test, was omitted, in part to save time, in part because it was desired to see whether the test could be used with safety without these devices for instilling an attitude of cautiousness in the student.

concatenate	lanuginous	skysail
concatenation	lanugo	skyward
concave	lanyard	tenderloin
conceal	muff	tendinous
disentomb	muffin	tendon
disentrance	muffle	tendril
disepalous	mufti	virago
disestablish	photograph	virescent
filiform	photographer	virgin
filigree	photography	

(2) To arrive at a correct vocabulary-index and to detect the presence of a tendency either to overestimate or underestimate, I required the students to write the definitions of forty words selected from the above list *after* they had completed the first test. This definition test was, of course, entirely unexpected by the students. It was intended to require the definition of every word that might possibly be unknown or doubtful, but, as events showed, to accomplish this result the list would have to be extended to embrace sixteen additional words.

The definition list, for which twenty minutes was allowed, was as follows: abductor, abet, baroscope, chanticleer, chaos, decemvirate, eschar, escheat, eschalot, gourd, interdict, interim, mattock, maturate, pudgy, scruff, scrunch, subcutaneous, tycoon, tymbal, amalgamation, amanuensis, amaranth, bottomry, concatenate, disentrance, disepalous, disestablish, filiform, hoecake, lanugo, lanyard, mufti, photo-lithograph, rejoinder, skysail, tendinous, tendril, virago, virescent. Concatenation, lanuginose and lanuginous can, of course, be checked off by the definitions given for concatenate and lanugo.

The words which experience showed should have been added were: abeam, abed, barque, barouche, boudoir, disentomb, filigree, hodman, pagoda, rejuvenate, scroll, sub-let, tenderloin, — a rather amazing list for college students!

Results. — In the vocabulary test proper, 70 students, aged 16 to 25 years, 16 men and 54 women, on the average, marked 77.6 per cent. known, 17.2 per cent. unknown, and 5.2 per cent. doubtful. The highest known was 92 per cent., the lowest 64 per cent.

In the second part of the test, the actual definitions of the 40 selected words, the results were treated in the following manner. A record was made for each student of (1) the number of words not defined, and (2) the number of words wrongly defined. (3) These were added to give the total number of words unknown in the list of 40. (4) By reference to the

vocabulary test the number of other words unknown beside those in the definition test was ascertained (this was from two to ten). (5) There remained in some instances (33) from one to three words each (averaging for all those tested 0.6) that had originally been marked doubtful, and which still remained doubtful because they were not on the definition list: these scattering doubtful cases were regarded as unknown. (6) By combination of all the unknown cases, it was then possible very easily to compute the final corrected vocabulary-index of each student, and (7) by comparison with the original markings, to determine the amount of over- or under-estimation.

The results, based on 70 cases, are as follows:

Average number of words in list of 40 not defined	15.50
Average number of words in list of 40 wrongly defined	6.64
Average number of words in list of 40 not known	22.14
Average number of words outside list of 40 not known	4.20
Average number of words left doubtful	0.60
Average corrected vocabulary-index	73.26 (m. v. 5.5)
Average amount of overestimation	4.37
Number of students overestimating	59
Number of students underestimating	10
Number of students neither over- nor underestimating	1
Largest overestimation	18
Largest underestimation	4

The decided nature of the tendency towards overestimation is more clearly evident from its distribution as given in Table I.

TABLE I.

OVERESTIMATION OF THE VOCABULARY-INDEX (70 COLLEGE STUDENTS).

Per cent. Overestimated.	Number.	Per cent. Overestimated.	Number.
18	1	5	7
15	2	4	6
14	3	3	7
13	1	2	10
12	3	1	9
11	1	0	1
9	2	−1	6
8	2	−2	2
7	1	−4	2
6	4		

It will be seen that, of 70 students, 11 overestimate by more than 10 per cent., and that 20, or more than one quarter of the

students, overestimate by 5 per cent. or more. From these figures it is evident that, without a somewhat elaborate definition-check, the valuc of the voeabulary-test is distinctly lessened.[1]

The corrected vocabulary-index is of sufficient interest to justify a brief presentation of its distribution (Table II.).

TABLE II.

DISTRIBUTION OF THE CORRECTED VOCABULARY-INDEX (70 COLLEGE STUDENTS).

Index.	Number of Cases.
85–89	5
80–84	5
75–79	19
70–74	22
65–69	13
60–64	6
55–59	1
Highest index,	89.0
Average index,	73.26
Lowest index,	58.0

Sex differences in vocabulary cannot with certainty be established from the small number of cases here considered, but the sixteen men average 75.8 and the 54 women 72.6 for the corrected index.

On the basis of Webster's Academic dictionary, circa 28,000 words, Kirkpatrick estimates the average vocabulary of college students at 20,120. From the present test, the students' own estimate figures 21,728, while their corrected vocabulary would be 20,512. The largest vocabulary would be 24,920, the smallest 16,240, or approximately that assigned by Kirkpatrick to the average second-year high-school student. Thus the wide individual variation in size of vocabulary to which Kirkpatrick calls attention is still further confirmed and emphasized.

The results of the definition-test are worth considering in

[1] This result may be compared with Kirkpatrick's conclusion that very young children are apt to underestimate because the isolated words of the list fail to arouse associations such as they would if they had a context. Again, when Kirkpatrick defined the words of the list to normal school students, he found that the errors of over- and under-estimation tended to cancel one another, while when college classes defined 20 words, 114 of 246 students (about 46 per cent.) correctly defined the same proportion that they had marked as known, and only 7 per cent. erred by as much as 3 in 20.

more detail. No word of the forty was correctly defined by every student, and, as we have noted, there were 16 other words that were unknown or doubtful. It follows, therefore, that *only 44 of the 100 test words were certainly known by every one of 70 college students.*

It is at times rather difficult to decide from the definitions whether the student does or does not know the meaning of a word with sufficient exactness to be credited with knowledge of the term in question.

To a slight extent, therefore, the ranking is arbitrary and might not agree with that of another experimenter who was using the same test. In general, however, I have erred on the side of leniency in consideration of the difficulty of accurate definition and of the short time (about twenty minutes) that could be expended upon the forty definitions. To illustrate, the following were accepted: 'disestablish — to overthrow,' 'decemvirate — a body of ten,' 'mattock — a garden tool,' 'amaranth — a flower'; while the following were disallowed: 'lanyard — one of the spars of a ship,' 'decemvirate — Roman civil officer,' 'gourd — a hollow vessel from which to eat and drink,' 'concatenate — to argue,' 'baroscope — an instrument for measuring something.'

Of interest, in this connection, are the erroneous definitions. These, as Kirkpatrick has pointed out, are most likely to result from the confusion of the given word with some word resembling it in sound or spelling. Other definitions are evidently sheer guesses from the fancied etymology of the word, *e. g.*, disepalous — without a head (from *dis* + *cephalous*). In the following list of typical errors of definition, the assumed source of confusion is indicated by the terms in parentheses after the definitions:

amanuensis — poet laureate, lovingness (amativeness).
amaranth — a precious stone (amethyst).
abet — although (albeit), a wager (a + bet), diminish (abate).
bottomry — the art of bottoming chairs, deceit, bottom of anything.
chanticleer — one who sings a loud song, one who leads a chant.
decemvirate — composed of five, count out by tens, formerly a group of ten men, but any number now.
disentrance — failure to enter.
disepalous — apart from the head, without shoulders.
gourd — reward (guerdon), to slash or whip (goad), morning glory.
interim — time between two reigns (interregnum).
lanugo — a kind of language.
lanyard — yard where leather is tanned (tanyard), yard about the lane.
mattock — a lock of hair (matted locks?), a kind of bird, a sort of rug, a kind of robe (cassock).

maturate — to ripen (mature), to matriculate.
sky-sail — a sail in the sky, a kite.
tycoon — a violent wind (typhoon), an animal, a silk-worm, a natural phenomena (sic).
tendril — a membrane connecting two bones (tendon).
tendinous — capable of endurance (tenacious?).
scrunch — a good for nothing person, (scrug?).
virago — a kind of bird (!) (vireo), a disease, giddiness (vertigo).
virescent — sparkling (iridescent), of or pertaining to a man (!) (virile).

B. A Word-Building Test.

The word-building tests were suggested to me by the familiar game of anagrams as well as by the advertisements often seen in magazines in which a prize is offered to the person who can make the most words from a given word or series of letters. This test is easily administered and evaluated: it is one that calls for ingenuity and active attention: it might fairly be said to demand that ability to combine isolated fragments into a whole, which Ebbinghaus has declared to be the essence of intelligence and for the measurement of which he devised his well known 'combination method';[1] and, finally, its execution is conditioned to a certain extent by the richness and readiness of the examiner's word-vocabulary. One may expect, therefore, to find a correlation between this test and the vocabulary test and possibly between it and school standing or general intelligence.

Nature of the Test. — After some preliminary trials, two word-building tests were arranged and distributed to the classes above mentioned in the form of mimeographed blanks which read as follows:

Word-Building Test, No. 1.

Make as many words as you can from the six letters given below. You may use any number of letters from one to six, but no letter may be used twice in the same word, and no other letters than these six are to be used. You will have five minutes.

a e o

b m t

[1] H. Ebbinghaus, 'Ueber eine neue Methode zur Prüfung geistiger Fähigkeiten und ihre Anwendung bei Schulkindern,' *Zeits. f. Psych. u. Physiol.*, XIII., April, 1897, 401–459.

Then followed numbered spaces for 30 words, and blanks for name, date, etc. The second test was exactly similar save that the six letters given were: *e*, *a*, *i* and *r*, *l*, *p*.

The possibilities of combination are larger than one might suppose: from the first at least 70, from the second at least 105 words may be formed. Proper names were allowed, but words in foreign languages were not allowed.

Test No. 1 was given to two groups of college students and likewise to a group of fifty boys from the seventh and eighth grades of the Ithaca public schools.[1] Test No. 2 was given to the college students only.

Results. — In Table III. the chief results of these tests are summarized. It will be seen that Test No. 2 offers more possible combinations, that more words are made with it and a greater

TABLE III.

Test.	Date.	Grade.	Male.	Female.	Largest.	Smallest.	Average.	Mean Variation.	Total No. Different Words.	Possible Words.
1	2/19,06	College	7	15	25	10	18.6	3.54	41	70
1	3/19,07	"	9	27	26	10	18.6	3.30	41	70
1	4/10,06	Grammar	50	0	21	6	12.4	2.37	38	70
2	3/ 6,06	College	7	26	32	14	21.9	4.00	57	105
2	3/19,07	"	9	27	33	15	23.5	3.66	61	105

number of different words. It will also be noted that the individual differences in rank are large; thus it happens that not a few grammar school boys make more words than some of the college students: to be more explicit, it may be stated that, with Test No. 1, 10 grammar pupils make 15 words or over, while 13 college students make fewer than 15 words.

By examining the papers in detail, and tabulating the total number of words formed and the number of times each of these words is given, one may discern something of the principles which govern the operation of the test. The following are the data thus secured:

TEST NO. 1. 58 COLLEGE STUDENTS. (45 DIFFERENT WORDS.)
Over 50 times — bat, mat, bet.
40–49 times — eat, met, Tom, at, boat.

[1] This was in connection with an extensive investigation upon the correlation of various physical and mental tests, the results of which I hope to publish soon.

30–39 times — meat, to, tea, beat, team, tab, ate, am, moat, mob, me, beam, toe.
20–29 times — tame, oat, be, mate.
10–19 times — boa, mote, bate, abet, tomb, tome, tam.
5– 9 times — Mab, Abe, Mae, ma, atom, a.
1– 4 times — bot, mot, o, Moab, beta, bema, tabe.
Not given[1] — ab, ambe, ambo, amt, atmo, ba, bam, bo, bom, boma, bote, ea, eam, eb, em, eta, mao, meta, mo, moa, moe, ob, obe, om, ta, tambo, tema,

TEST NO. 1. 50 GRAMMAR GRADE BOYS. (38 DIFFERENT WORDS.)

Over 40 times — mat, bat.
30–39 times — bet, at, met.
20–29 times — to, eat, Tom, beat, tea, meat, be, am, boat.
10–19 times — toe, mob, beam, me, ate, team, tab, boa, oat.
5– 9 times — ma, bate, a, moat, mot, tame, mate, bot.
1– 4 times — tam, tomb, Abe, mote, Moab, Mae, o.
Not given — those not given by college students, plus abet, atom, bema, beta, Mab, tabe, tome.

TEST NO. 2. 69 COLLEGE STUDENTS. (66 DIFFERENT WORDS.)

Over 60 times — lip, lap.
50–59 times — rip, rap, pear, ear, real, pie, leap, rail, pale, reap.
40–49 times — reap, pail, pile, ale, pair, are, ape, lie, pea, peal.
30–39 times — pare, earl, pearl, air, par, lair, ripe, liar.
20–29 times — ail, Lear, rape, ire, pal.
10–19 times — lea, pa, rile, pire, era, pier.
5– 9 times — per, a, alp, Eli, plea.
1– 4 times — I, paler, peril, lira, rep, rale, ile, lare, ra, pil, piler, ril, April, Ira, la, pareil, pi, pilar, Rea, Rae.
Not given[1] — ai, aiel, aile, aire, al, apl, ar, Ariel, aril, ea, el, ela, epi, er, eria, il, irp, le, lep, lepra, lerp, li, lier, lire, lirp, paie, pali, parel, parl, pela, pel, pia, piel, pila, plie, plier, prial, prie, re, rei, rial, ril, ripa.

Inspection of these lists shows (1) that three-letter words are in every instance those most frequently formed, (2) that two-letter words and the one-letter words, which one might expect to be most frequent since most simple, stand relatively low, *e. g.*, *ma*, *be*, *am*, *pa*, *me*, *a*, *o*, *I*,[2] (3) that grammar school boys give all the words given by college students save a few rather unusual terms such as *atom* and *tome*, (4) that usage and ordinary speaking vocabulary condition the formation of words, in as much as the most ordinary words have the greatest fre-

[1] This list is based on the words actually given in the Standard dictionary, not including, however, Scotch terms.

[2] It appeared, upon inquiry, that some of the college students had omitted words like *pa*, *ma*, *a*, *o*, and *I* on the ground that they were 'not real words,' or 'didn't count,' but, oftener, they seem to have been passed over because the attention was concentrated upon the making of *combinations*.

quency, *e. g.*, *bat*, *mat*, *bet*, *eat*, *lip*, *lap*, whereas words that are less frequently used in every-day speech, although their meaning is doubtless perfectly well known, do not suggest themselves so readily under the conditions of the test, *e. g.*, *tomb*, *tome*, *era*, *plea*, *paler*, (5) that the words not given by any one are, with one or two exceptions, *e. g.*, *plier*, words of extremely rare usage or unusual form, alternative spellings, etc.

Sex differences may not with certainty be made out, yet, as Table IV. indicates, in every group tested, the men did slightly better than the women: though this difference is small — less than the mean variation, yet it is constant in direction in all four instances.

TABLE IV.

SEX DIFFERENCES IN WORD BUILDING.

Test.	Date.	Men.	Average.	Women.	Average.
1	1906	7	18.7	15	18.6
1	1907	9	19.7	27	18.0
2	1906	7	23.6	26	21.4
2	1907	9	25.8	27	22.7

C. CORRELATIONS.

In the case of the college students I have been able to test the correlation of vocabulary and word-building test with one another and of each test with the class standing in Educational Psychology. This class standing was based upon marks obtained in examination and upon class exercises, and was secured with suitable precaution, at least in the case of the examinations, to render it absolutely objective.

With the grammar-school boys, it was possible to compute correlations between word-building and numerous other tests. Save for class standing the latter are reserved for a later paper. All of the correlations were worked by the well-known Pearson formula, save that when the distribution was symmetrical, the value of sigma (the standard deviation) was computed from the average deviation by multiplying by the constant 1.2533 — a procedure which cuts the work of computation by one half and gives values closely approximating those obtained by the usual method of computing the standard deviation.

TABLE V.

CORRELATIONS.

Data Correlated.	Cases.	r.	p.e.
Vocabulary index and word building	58	+.53	.05
Vocabulary index and class standing	58	+.45	.06
Word building and class standing	58	+.13	.07
Word building and class standing	50	+.08	.08

D. SUMMARY.

1. The determination of the vocabulary-index, as proposed by Kirkpatrick, by means of a list of 100 words selected by chance is clearly influenced by a tendency to overestimation when no precautionary restrictions are employed. In tests of college classes, 15 per cent. of those tested had an overestimation error amounting to 10 per cent. or more, and 25 per cent. of those tested had an overestimation error of 5 per cent. or more.

2. By check definition-tests it is possible to eliminate or to measure this error of overestimation. When such a check was applied, the average vocabulary-index for college students was found to be 73 per cent., with a maximum of 89 per cent. and a minimum of 58 per cent. This minimal index is less than that assigned by Kirkpatrick for the average second-year high-school student, and illustrates forcibly the unevenness of the mental equipment of college students.

3. The definition-test reveals an unexpectedly large number of erroneous definitions. The source of these errors may frequently be traced to confusions with words of similar appearance or to fancied etymological derivations.

4. Simple word-building tests correlate fairly well with the vocabulary test. Marked individual differences are apparent here, as in the vocabulary test; so marked, indeed, that some grammar-school pupils excel some college students in the construction of these lists.

5. An inspection of the lists of words thus constructed reveals the psychological conditions which affect the process. In general those words are most frequently given that are in most common use in the daily speaking vocabulary, whereas

words that are equally well known but in less frequent use are apt to be omitted.

6. Sex differences cannot be clearly established in these two tests, but what differences appear are constant in direction and suggest the superiority of men and boys over women and girls.

7. The vocabulary test correlates fairly well with class standing, but the word-building test rather unexpectedly shows no such correlation.[1]

[1] The MS. of this article was received November 15, 1907. — ED.

THE DOCTRINE OF PRIMARY AND SECONDARY SENSORY ELEMENTS. (II.)

BY DR. BORIS SIDIS,

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VIII.

In the cases of hallucinations investigated by me I have found pathological processes which gave rise to secondary sensations crystallized into hallucinations. Thus one of my cases suffered from auditory hallucinations. The patient heard voices telling her all kinds of disagreeable things. She complained that the voices came not through the ear, but through a spot located over the Fallopian tubes. An examination of the ear showed nothing abnormal. Physical examination revealed nothing abnormal in any of the other sense organs. The Fallopian tubes, however, were very tender and painful to pressure. The patient suffered from an old chronic salpingitis. The hallucinations, which were of a sexual character, became more severe at regular intervals coinciding with monthly periodicities.

One case of mine suffered from visual hallucinations. He saw spirits, ghosts and visions of saints. When he travelled in a car, he could see little men with benevolent faces and for some religious reason he regarded them as saints who came to his help. He could see them splitting the rocks and disappearing there, or sometimes the rocks split open and the saintly little men came to the surface. Occasionally apparitions of the dead visited him. The visions were never quiet, but always in motion, they did not stay long and rapidly disappeared, giving rise to new visions. An examination of his special sense-organs showed nothing abnormal. The sense of touch, pressure and kinæsthetic sensibility manifested peculiar abnormalities. The skin of the body was very sensitive and that of the scalp was extremely tender to touch. The patient could not bear any pressure of the scalp and was mostly bareheaded, though he was

very sensitive to draught and to changes of temperature. Occasionally he experienced a sense of formication all over the body, especially in the scalp and in the region of the neck, the muscles of which were extremely sensitive to pressure. Now when the head was inclined to one side or pressed hard or kept in tense state for a couple of minutes at a stretch, he could see spirits floating in the air, he could see the little men with their saintly faces coming out of the ground and disappearing into it again.

One case of functional psychosis, with epileptiform attacks presenting phenomena of dissociated states with distinct tendencies toward the formation of multiple personality, suffered a good deal from auditory hallucinations. It will take too much space to give an account of the details of the different seizures and of the various dissociated states manifested by the patient. For our present purpose it is enough to refer to the hallucinations. The patient complained that she could hear voices talking to her, her mother and brothers communicating with her from a distance. An examination of the auditory apparatus proved it to be in excellent condition. Now in this case the phenomena of unconscious phonation were quite well developed, the patient was observed to move her lips and whisper — the whisper becoming sometimes quite loud so that many words which the patient referred to the voices of the mother and brothers were really uttered by the patient. An examination of the eye revealed the presence of an astigmatic condition and a limitation of the field of vision. When the patient was made to count or to read aloud or when absorbed in a conversation, the auditory hallucinations ceased. The auditory hallucinations considerably diminished, both in frequency and intensity, when the astigmatism was corrected by eye glasses.

Similarly in another case the patient suffered from auditory hallucinations. Here the patient was observed talking to himself. This was so pronounced that now and then he himself became conscious of the fact that he was talking to himself. He describes this experience of automatic talk which seems to be uncontrollable and of which he is often unconscious by the term of 'autovocalization.' In this case the patient now and

then can catch himself telling things to himself which he takes for the voices of other people as he is then conscious of the hearing, but not of the utterance of the words and phrases. This, however, is not always the case; in fact in a good many cases where unconscious phonation is present, as, for instance, in the case of the patient with the epileptiform seizures described above, the patient is entirely unconscious of the fact of 'whispering.' When attention was drawn to the phenomenon, the whisper and the hallucination disappeared.

One of my cases, a lady of about sixty, suffered for about fourteen years from auditory and visual hallucinations. She complained that she was surrounded by ghosts of departed family-members who did not leave her alone. The spirits talk to her, they give her advice which is often against her interests. Her departed husband and his brother are the chief leaders, the 'guides' so to say. They talk to her on all important occasions and try to guide her in life. The patient resents such interferences. There is no need to go here into the details of the case. When the voices became insistent she also had visions of the spirits and could hear them talk to her, a proceeding which she always attempted to discourage, but she admitted that the voices and the spirits had the best of her and she was forced to follow their instructions. Now an examination of the patient revealed the fact that the hearing on the left side was rather defective, the tympanic membrane was thickened and there was present a chronic pathological process due to a former condition of middle ear disease. Any continuous and prolonged irritation of the diseased ear started the voices, increased their intensity, and caused the manifestations of the visions.

I may also refer to a patient under my care who suffered from auditory hallucinations and thought herself possessed by demons. From her ninth year she suffered at various intervals from those voices which sometimes told her quite unpleasant things. Along with the hallucinations she also had attacks of automatic speech. Now and then she simply heard voices and was not conscious of any involuntary speech, but occasionally the involuntary utterance took such possession of her that she could not control it. She felt as if some other being got possession of her organs

of speech. This frightened her even more than the hallucinations. She kept away from her friends fearing sudden attacks of involuntary speech. It appeared to the patient, as if some other beings made her talk against her will. She shunned society, because the other beings forced her to tell aloud what she thought of the people in whose company she was present. When she was not conscious of the forced speech, she often heard voices which she ascribed to the same demons. There was nothing of the delusion of paranoia in it as she could not account for the involuntary speech and auditory hallucinations. The patient was of Irish descent and uneducated though very intelligent, and the explanation of 'demoniacal possession' was given and maintained by her family in Ireland. She was glad to take my view of the phenomena which I tried to make plain to her, as much as it was possible under the circumstances. A quotation from her written account may be of interest: "When I was nine years old, one day I remember I sat down on a stone and suddenly I heard a voice: 'If you live four or five years more you will wish you had never grown up.' I thought it was strange, but soon forgot it and went to play again. I had no trouble until I was fourteen, when the voice changed and forced me to talk with my own voice. The voices would make me speak of things that in my own self I had no idea of doing and would not do for anything. About eight years ago I had a terrible fright after which I thought I talked with saints and angels and saw unusual things, I really saw them." We find here the presence of automatic speech, unconscious phonation with subconscious states resulting in dissociations of secondary from primary sensory elements with the consequent formation of various forms of hallucinations.

IX.

I should have liked very much to bring in here some of the work on hallucinations carried out by Dr. William A. White, but space requirements forbid. The cases studied by Dr. White are extremely interesting and go to substantiate the theory of hallucinations advanced by me. By a close study of a series of cases he sustains the validity of the present theory of hallucina-

tions. Dr. White's studies are of great importance to normal and abnormal psychology, and the reader who is interested in the subject I refer to his original contribution.[1]

Observations and experiments incontestably prove that hallucinations are synthetized compounds of secondary sensory elements, dissociated completely or incompletely from their primary elements.[2] Normal and abnormal perception do not differ psychologically as to their make-up, except in the relation of their primary and secondary sensory elements. Hallucinations are not central; they are essentially of peripheral origin; they are induced by peripheral excitations giving rise to peripheral physiological processes awakening primary sensory elements which are subconscious or fall out entirely of the patient's consciousness leaving the groups of secondary sensory elements to stand out as fully developed hallucinations. The hallucinatory secondary sensory elements may be tinged with the qualitative aspect of the dissociated primary sensory elements, thus pathological processes in the auditory sense organ may give rise to voices; or morbid processes of the visual apparatus may give rise to visions. Quite often, however, the dissociation is so deep and extensive that the synthetized system of secondary sensory elements does not bear the least trace of the qualitative aspect of the primary sensory elements; thus a morbid condition of the pharynx, for example, may give rise to an auditory and even to a visual hallucination. Whatever may be the qualitative character of the sensory compounds one thing stands out clear and distinct, and that is the fact that the percept, whether normal or abnormal, does not consist of images, but of sensations, primary and secondary.

X.

Contrary to the general view maintained by most psychologists we have laid special stress on the fact of the fundamental qualitative difference between image and sensation. We shall not venture far from our facts, if we arrange images and sensations in two qualitatively different psychic series. Sen-

[1] See *Journal of Nervous and Mental Disease*, 1904.

[2] It gives me great pleasure to add that Professor James in a note to me finds the theory advanced fully convincing as it is substantiated by the facts.

sations can be ranged in a graduated series of *intensities*, while images or representations can be ranged in a graduated series of clearness and distinctness, or of *vividness* as it is sometimes described by some psychologists. I use the term vividness in the sense of clearness and distinctness and not in the sense of intensity as it is often used; even those psychologists who do not use intensity and vividness indiscriminately ascribe both of them equally to sensation and image. Now vividness and intensity are understood by me to be two fundamentally qualitatively different aspects, or attributes. Sensations have intensity, but no vividness; images or representations have vividness, but no intensity. Sensory elements may vary from *minimum* to *maximum* intensity. This variation in intensity holds true both of primary and secondary sensory elements. Similarly, images or representations may pass through all degrees of vividness from *minimum* to *maximum*. The image represents the sensation. In this respect we may somewhat modify the well-known dictum of the sensationalists into: 'Nihil est in imagine quod non antefuerit in sensu.' The sensory element is symbolized by its respective representative element. Now the representative elements may refer with different degrees of vividness to the same sensory elements. An image with one degree of vividness can be substituted for another with a different degree of vividness and still refer to the same sensory elements. The degree of vividness does not change the qualitative character of the representation. Not so is it with the qualitative attribute of the sensation. The slightest change in the intensity of the sensation changes its qualitative character. A sensation with one degree of intensity cannot be substituted for another. A sound or a color of a definite intensity cannot be substituted for a sound or color of a different intensity. The two are different sensations and no sensation can substitute another. Sensations falling in the same series of intensity are really independent of one another, but each sensation of the intensive series can be represented by a whole series of representations of different vividness, from *minimum* to *maximum*. Different series of representative elements may also be regarded as independent, since they refer to independent sensations.

If we symbolize a series of sensory elements by the letters: $A_1, A_2, A_3, A_4, A_5, \ldots A_n$; and if we symbolize the corresponding series of representative elements by $a_1, a_2, a_3, a_4, a_5, \ldots a_n$, then the series of both sensory and representative elements may be symbolized by the following formula:

$$\begin{array}{cccccccc}
A_1 & A_2 & A_3 & A_4 & A_5 & \ldots & & A_n \\
a_1 & a_2 & a_3 & a_4 & a_5 & & & a_n \\
a_1^1 & a_2^1 & a_3^1 & . & . & & & . \\
a_1^2 & a_2^2 & a_3^2 & . & . & & & . \\
a_1^3 & a_2^3 & a_3^3 & . & . & & & . \\
a_1^4 & a_2^4 & a_3^4 & . & . & & & . \\
a_1^5 & a_2^5 & a_3^5 & . & . & & & . \\
. & . & . & . & . & & & . \\
. & . & . & . & . & & & . \\
. & . & . & . & . & & & . \\
a_1^n & a_2^n & a_3^n & a_4^n & a_5^n & & & a_n^n
\end{array}$$

The characteristic of the image, or of the representative element is just its extraordinary plasticity and possibility of substitution. This function of substitution was described by Taine with all the power of his lucid style. The great modifiability of the representation plays an important rôle in psychic life — adaptability to various conditions of life increases, reactions cease to be rigid and uniform, but change easily in response to a changing environment. Variations of sense-organs with their physiological processes are rather slow and tardy, often requiring generations for an effective change, while the representative element can be modified and adapted within the life-existence of the individual and often in a very short time. In brief, the function of substitution possessed by the representative element in the process of mental selection is the substitute for natural selection in the highest representatives of animal life.

Now under ordinary conditions of life the gradated series of representative vividness runs parallel to the gradated series of sensory intensities. Usually a more intense sensation is represented with greater vividness. The increase or decrease of intensity of the sensory series has a corresponding change in the vividness of the elements of the representative series. Intensity and vividness vary directly. Such direct variation, however, is not always the rule. There are cases, when the two part

company. In states of distraction, in subwaking states, in states of dissociation and generally in the conditions of functional psychosis, intensity and vividness do not vary directly. Strong stimulations may give rise to sensations of great intensity, but the vividness of the representative elements may fall so low as almost to reach the minimum. When the vividness is so low as to reach the *minimum*, the representative elements cannot be used as substitutes and, since reproduction belongs to representative elements which symbolically reproduce the sensations by the process of substitution, reproduction or memory of the original experience is absent and there is a break, a gap in mental continuity, dissociation results. The depth and extent of dissociated mental systems may be regarded as variables of vividness. Dissociation varies inversely as vividness. When vividness is at its *minimum*, dissociation is at its *maximum*. The phenomena of functional psychosis having their origin in states of dissociation may thus be regarded psychologically as functions of vividness, the most characteristic attribute of representative elements. Functional psychosis with all its protean manifestations, the great variety of dissociated and subconscious states may thus be reduced to variations of one fundamental attribute — vividness.

XI.

We must not omit to point out another fundamental difference between sensory and representative elements. Sensations have the significance, or possess the attribute of external reality, while images, ideas or representations entirely lack it. Put in Professor Baldwin's excellent terminology — sensations have the coefficient of external reality, the sensory coefficient of reality. No matter whether the sensation was produced by an external stimulus, or by a pathological process going on in the sense-organ, or brought about indirectly through the action of another sense-organ by means of indirect association-paths of neuron-collaterals, no matter whether the sensation is primary or secondary, as long as it is a sensation at all, it possesses the sensory coefficient of reality. A sensation whether 'true or false' possesses rightfully the coefficient of reality as its necessary and inherent attribute. The percept, true or hallucinatory,

consisting of sensory elements has therefore the sensory coefficient of reality.

Psychologically regarded, the 'true' percept and the hallucination have the same sensory constitution with the same attributes. The difference between the true and false percept may be regarded from a biological standpoint as a matter of adjustment. The percepts with successful adjustments are true, while those with unsuccessful motor reactions are false and hallucinatory. Psychologically, the difference between the 'true' percept and hallucination is in the *shifting* of the primary and secondary sensory elements. *Where the secondary sensory elements can be shifted and become primary, the percept is regarded as true; where the secondary sensory elements do not admit of being shifted and become primary, the percept is regarded as hallucinatory.*

If we turn now to the representative elements, we find that they lack the sensory coefficient of reality. This lack of sensory coefficient is only the negative side of the image. There is also a positive side to it. The image is not felt as image simply because it is not sensation or lacks the sensory coefficient, but because it possesses a qualitative character of its own. A sensation is not felt as such simply because it lacks the character of another sensation. Thus sensation green is not experienced as the particular color sensation, because it has not the *quale* of sound or of pressure, but because the sensation green has a positive experience of its own. The same holds good of the representation — it possesses its own characteristic *quale*. As an experience *sui generis* we claim for the representation a special psychic mark, an 'ideational or representative' coefficient. The image has its own qualitative character just as the sensation possesses its own. In contrast to the sensation which possesses the coefficient of external reality, the image or representation has the coefficient of internal reality. Both sensation and image have reality, each one has its own kind of reality — the sensation has external *objective* reality, the image has internal *subjective* reality. It is on account of the ideational or representative coefficient that every image is placed unhesitatingly into its own world of reality, into its own series of images with which it easily associates and fuses.

Writers on psychology in trying to define further the coefficient of reality refer it to the will. Some maintain that the coefficient of reality is the 'independence of the will,' while others claim that the coefficient of reality is 'subjection to the will.' Professor Baldwin in his paper, 'The Perception of External Reality,' offers an extremely interesting solution which reconciles both views. He points out that there is a difference between the 'memory coefficient' of reality and 'sensational coefficient' of reality. The two coefficients are opposite as far as control of will is concerned. The sensational coefficient is independence of the will, while the memory coefficient is control by the will. A sensation, in short, is not under the control of the will, while an image is subject to the will. He makes a further distinction between a simple image or 'memory image' and a 'memory image of external reality.' The memory image can be brought up voluntarily by its proper associates, but it has no sensational coefficient as a result, while the memory image of external reality can be followed by sensational coefficients, that is, sensations can be brought about in the train of such an image. To quote Professor Baldwin: "Certainly a present sensible reality is not under the control of my will; it is independent, and if my coefficient is to be discovered in the relation of the presentation to my voluntary life, this must be its expression and I go over to the class of writers who find the psychological basis of external reality in sensations of resistance. But when we come to inquire into the 'memory' coefficient — asking the question what character is in a memory-image which testifies to its being a memory of reality? the tables seemed to be turned. Without stopping to examine other views, I hold that that image is a true memory which we are able to *get again as a sensation* (Professor Baldwin's italics) by voluntarily repeating the series of muscular sensations which were associated with it in its first experience. The memory coefficient therefore is subjection to the will in the sense indicated. . . . A true memory in short is an image which I can get at will by a train of memory associates, and which, when got, is further subject to my will; a memory of external reality, on the contrary, is an image which I can get at will by a train of sensational associates and which, when got, is not subject to my will."

Now if I understand Professor Baldwin aright, a sensation does not fall under the control of the will, while a simple 'memory image' and a 'memory image of external reality' are both under the control of the will, the difference being that the former does not terminate in a sensation, whereas the latter does. This I take to mean that a sensation does not depend on the subject (will), but on the external object; in other words, a sensation cannot be produced from center to periphery (not internally initiated by the will), but is initiated by an external excitation peripherally stimulating the sense-organ and giving rise to sensation. An image, on the other hand, does not depend for its initiation on the external object or excitation, but is essentially an internal event which can be brought about from within by the process of associative activity so highly characteristic of the image. Thus far my analysis seems to me to be in full accord with Professor Baldwin's view. Similarly, Professor Baldwin's views in regard to 'memory images' and 'memory images of external reality,' the former not ending in sensory experience, the latter terminating in experience with sensory coefficient, seem to me to be closely related to the views expressed by me in this paper and in my other works on the subject.

In spite of the agreement on so many points there are other points which do not appear to me acceptable. We may agree that kinæsthetic and muscular sensations or sensations of resistance are at the core of things, but are they the be-all of external reality? Have not sensations of pain, of hearing, of color, or of smell as much reality as our sensations coming from muscle, joint, synovial membrane and articular surfaces? The acute, shooting, twinging pains of rheumatism, gout, tabes-dorsalis, the burning pains of meningitis, the excruciating throbs of megrim, the fine stabbing pains of toothache, the agony of angina, the sharp tormenting pains of facial neuralgia and many other pains coming from different organs and tissues, are not they real and external? In fact do they not bear on them more the mark of grim, pitiless, external necessity than any of the sensations coming from acitve muscle and joint? What about light, color, sound, smell, are not they sensations of ex-

ternal reality, even if sensations of resistance do not enter into their make-up? Muscular and kinæsthetic sensations may be granted to play a very important *rôle* in our knowledge of things, but psychologically regarded, all sensations bear on them unmistakably the mark of external reality. It is not the particular form or kind of sensation, but it is the sensory *quale* as such that gives the coefficient of reality. As far as resistance is concerned Professor Baldwin is right, if it be applied to each and every sensation. For each and every sensation possesses this mark of stubbornness about it; it shows opposition, resistance and floods the mind. We may say that the stimulus forces open the gates of the sense-organs and invades the mind with an overwhelming power. Still, on the whole, Professor Baldwin is right in laying special stress on sensations of activity (?) and resistance since, biologically regarded, they are the ones that give the smack of life and the kernel of things and help to bring about adjustments to the external environment.

Thus far the difference between Professor Baldwin and myself seems to be rather insignificant.[1] When, however, we reach what Professor Baldwin terms the 'memory image of external reality' the difference stands out somewhat more strongly. He contrasts the two, image and sensation, on the basis of dependence or independence of the will. The sensation is independent of the will, while the memory image of external reality is subject to the will which *can bring about the sensation originally experienced*. Now it seems to me that we are just as sure of the external reality of a sensation referred to by the memory image, even if we *cannot* bring about the original experience. We may perceive sensations which cannot

[1] The difference is far less than I have originally thought. In a letter to me Professor Baldwin writes: "I am much interested in your views. You will find my later and fuller treatment of resistance and of the nature of memory images in my *Thought and Things, or Genetic Logic*, where I attempt explicitly to trace the genetic development of knowledge from sense objects to image objects in detail, being I think nearer to your views than my earlier article brought out." I have since made myself acquainted with Professor Baldwin's great work on the genesis of knowledge, which I find to be of the utmost importance to psychology and epistemology. Much as I wish to discuss that masterly work in connection with my present theory, I find it impracticable on account of the lack of space. I hope, however, to return to the subject in another paper.

possibly be repeated and still they are regarded in memory as events that have taken place in the world of external reality. We may have the perception of a comet which may never again come into our experience, and even if it should come its coming is not due to our voluntary control; it is not *we* that can make the comet-experience come into our perceptual or sensory world with its sensory coefficient of external reality. We may be in the position of Plato's cave-dwellers and have no control over the reality, the reflection of which is displayed before us, and still we may agree with Plato that for the cave-dwellers the memory images of external reality, the recurrence of which is not under control, will still be discriminated from a general memory image, from an image of fancy. The sensation or percept may be unique, its reproduction may not be possible and still its memory image will be that of external reality.

On the other hand we meet in psychopathology with a vast domain of phenomena, such as recurrent mental states, insistent ideas which force themselves on the patient's mind against his will. The recurrent mental states or the insistent ideas are far more stubborn and uncontrollable than any resistant sensory object. The ideas may come like attacks which overcome the patient more than any sensory reality, or the idea may be persistent gnawing at the very vitals of his mental life. No external object is so stubbornly, so painfully resistant as just such an idea; and still the insistent idea is not regarded as a sensory reality. The insistent idea possesses the coefficient of external reality, independence of the will, painfully so, and still it is not regarded by the patient as external reality; in spite of its being independent of the will, it is still regarded as an idea.[1] It seems to me that we cannot express the sensational and ideational coefficients in terms of will of control or non-control. It is not resistance to the will that makes experience sensory, nor is it subjection to the will that makes experience ideational or representative. Why not state the fact as it is, external reality is the *quale* of sensory experience, while internal reality is the *quale* of the image or representation? A sensation is experi-

[1] Professor Baldwin admirably discusses this point. See Baldwin, *Thought and Things*, Vol. I., Ch. X., § 2.

enced as sensation, no matter whether or no it depends on the will, the independence is a secondary matter; the same is in the case of the image, it is experienced as image, independent of the fact of its subjection to the will.

XII.

There is another view which finds the fundamental difference between percept and image in what is and what is not common to all selves. Perceptual experience is common, while ideational experience is not common to all fellow-beings. I see the sun and other people can share it with me, while my image of the sun is experienced by myself. Thus one writer[1] tells us: "I perceive lowering heavens, pouring rain, bare trees and drenched sparrows, but I imagine wide horizons, brilliant sky, blossoming apple-trees and nestling orioles. The main difference is this: in the one case I assume that my experience is shared by other people and that everybody who looks out sees the same dreary landscape; but my imagination of the sunny orchard I regard as my private and unshared experience." Now the mark of being common is not the essential coefficient of external reality given by the percept. The percept is not experienced as external, because it is common to other people. We do not see the tree yonder, because other people can see it too; we would see it there, even, if, like Robinson Crusoe, we had no fellow-being to compare notes with. A hallucination is as fully a percept and is perceived in the full garb of external reality, although it may have no currency with my fellow-men. The percept possesses the coefficient of external reality, no matter whether or no others can share in it.

Moreover, psychologically regarded, the percept is as much of a private experience as the image is; in fact every psychic state has the privacy ascribed to the image and as such is unshared by other selves. It is simply the old psychological fallacy of confusing the physical with the psychic object or with the psychic state cognizant of the physical object.[2] The flower

[1] Mary W. Calkins, *An Introduction to Psychology*.

[2] Royce and Münsterberg define the physical object in terms of 'sociality,' but if I understand them correctly they do not regard the definition as a psychological one.

as physical object, as stimulus is shared by all who perceive it, but the perception of the flower varies with each individual. My perception of the flower cannot be experienced by any one else; like the image, the percept is entirely individual, unshared by other selves. I perceive the flower as having external reality not because my perceptual experience is the same as that of other people, nor because it is shared with others — as a matter of fact, it is *not the same* and from its very nature *cannot* be the same as the experience of others, as we *cannot possibly share* our individual psychic experience with our fellowmen. We perceive the flower as an external reality simply and solely because it is *sensory*. The percept consisting of sensations, primary and secondary, bears the impress of external reality; it possesses what Professor Baldwin so aptly terms 'sensational coefficient' giving external reality. External reality is given directly and *immediately* by the sensation or by the sensory compound, by the percept.

To quote from a former paper of mine: "Sensation carries along with it the reality of its stimulus. It is not that the sense of reality is different from the sensation, it is given in the sensation itself. Similarly the percept and the sense of external reality are not two different things; they are given together in the same process of perception and are identical. . . . The sensory process is also the process of the sense of external reality. . . . In seeing or perceiving the chair yonder we do not perceive it as real, because of its social or common character — the reality of its existence is given directly in the sensory processes of the percept itself. . . . The sense of reality of the external object is strengthened by association of the original sensory systems with other sensory systems, and the intensity rises in proportion to the number of systems of sensory elements, brought into relation with the functioning sensory systems. . . . The more systems of sensory elements are pressed into service, the stronger is the sense of external reality and the more assured is the reaction to the stimuli of the external environment. In the evolutionary process of man's adaptation to his environment he becomes extended in being and grows more developed, because of his social relations. Man presses into active service

the systems of sensory elements of his fellow-beings. Adaptations and hence successful reactions to the external environment are now more assured and the sense of external reality is still further emphasized and intensified. Throughout the course of intensification of the sense of reality the principle remains unchanged in nature. The sense of reality is given by and consists in nothing else but the sensory elements." From a philosophical and epistemological standpoint the social aspect may perhaps be sufficient to fix the externality of the object, but from a psychological standpoint the trade-mark of 'shares and common stock' has no currency. The percept consisting, as we have shown, of sensory elements, primary and secondary, possesses, on that account, the sensory attribute of external reality.

MEMORY FOR PAIRED ASSOCIATES.[1]

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The experimental studies of memory have been almost without exception studies of the retention of *series*, such as lists of figures, words or nonsense syllables and sections of poems or prose passages. The present study deals with the retention of the connections involved in paired associations, connections whereby, when the first member of a pair is given, the second can be supplied from memory. This memory of paired associations, or of isolated connections, may be defined in terms of the series type of memory as memory for a series of two terms when the first term is given.

In ordinary life such memorizing appears in such cases as connecting certain names with certain objects, faces, places, books and the like, connecting the appropriate words of one language with the words of another, connecting certain figures with $7 + 2$, 3×6, $19 - 13$ and the like, connecting events with dates, places or names, and so on through the practically countless number of cases where some one thing given should call to mind some other not given.

The data which are at the basis of my report consist of records of the objective achievements and subjective experiences of twenty-two adults, seniors or graduate students, in learning the English meanings of 1,200 German words, or of so many of them as they did not already know.

The list was made up without special care, though (1) words most likely to be known words, such as *ja*, *nein*, *zwei*, *Himmel*, (2) easy compounds of words already in the list, and (3) words whose etymological relation to English made them easily guessable, were in general excluded. There were some mistakes in spelling and occasional obsolete meanings. The nature of the list may be estimated from a random selection of 100 words from it given below.

[1] The MS. of this article was received Nov. 7, 1907.— Ed.

For purposes of study the 1,200 words were arranged in a study list of 120 numbered sets of 10 each, each set being typewritten in a vertical column, a German word and its English equivalent being on the same line in the order (from left to right) of German-English. For purposes of a test-list the 120 sets were retained with their numbers, but the order within any one of them was made entirely random with respect to the order in which they appeared on the study lists. Only the German words were given, of course. There was also arranged for purposes of testing a random selection of 120 of the German words, which had no correspondence with the arrangements by sets.

Gesellschaft	besiegen	finster	billig
Neid	bedauern	Besuch	Gebilde
Grund	fangen	flach	albern
bedenken	erbieten	eintreten	gewahren
Abscheu	fromm	Speise	Krebs
Loch	Gemüt	erkennen	Lump
schlank	Pfau	Kunde	Flur
schlagen	Lein	Wehmut	Flieder
Waffe	Nadel	Gegenteil	kleben
Höhle	fressen	Dienst	Abend
Lektüre	Kauel	Schweisz	innig
deutlich	Hecht	gnädig	ganz
nutz	Eule	plaudern	leifern
meiden	Mensch	vergraben	Hummer
Hippe	Dieb	Last	Knast
Kanapee	Reich	anfangen	gelten
Gemüse	streng	zittern	Enkel
Ort	Grimm	beleidigen	Erz
Falbel	Geschlecht	Auge	Falle
Haupt	umsonst	Wesen	merken
einfallen	schieszen	lass	Kantel
Bote	Tafel	Eidechse	dürfen
Geschichte	necken	Eidam	hehlen
führen	leugnen	Filz	leicht
fruchtbar	Wache	Hirsch	Heirat.

In order to secure enough subjects willing to undertake to spend thirty or more hours in memorizing vocabularies, I had to forego the desirability of having only such as knew no German at all. In a test at the beginning of the experiment with the 1,200-word list my subjects gave substantially correct meanings (liberally interpreted) for from 2 to 345 words, the detailed records being those of column 1 of Table I.

After this preliminary test the study list and the following instructions were given to each person:

"For the home tests the instructions are:

"Each training period to be approximately 60 minutes.

"*First.* — Get the materials needed, viz., pen or pencil, scrap paper, vocabularies to be studied, vocabularies to test with at end of hour and paper (a long sheet) for this test.

"*Second.* — Note the exact time in minutes when you are to begin to study the vocabularies. *E. g.*, 7.10.

"*Third.* — Study the vocabularies by whatever method you think the best. (You are to be entirely free to change your method, use several or do anything you please so long as you try always to make the best record possible, to do your utmost.) Notice the time occasionally so as not to run over the hour far. When an hour or approximately an hour (try never to work over 65 minutes or under 55, but do not be incessantly looking at the clock) has passed, stop studying the vocabularies, record the time of stopping. Put the vocabularies away.

"*Fourth.* — Take immediately the pages with only German words, place your test sheet of paper beside it and write for each German word its English meaning, numbering each such list with its proper number. Write on only one side of the paper. If you cannot make any likely guess, even, put a dash instead of the English word. Plan to spend not over 30 minutes in doing this. Record the number of minutes spent. Record the month, day of month, and time of day, your name, and the number of the practice period.

"In each practice hour 10 vocabularies, making 100 words, are to be studied. You may distribute your effort somewhat equally over the entire 10 or you may work with one 10 till you feel fairly sure of them, then with another 10 and so on, or you may study them in any way that you like.

"Twelve practice periods or six days will thus cover once the 1,200 words. With the thirteenth practice period you will use again the first 10 vocabularies, and so on for a second drill for practice periods 13–24, or six days more. With the twenty-fifth practice period you will begin a third time the vocabularies now twice studied and so on through a fourth and fifth expe-

rience. In case you find in these later experiences that the words are all perfectly known, never mind that fact but study them just the same.

"Some of the words may be misspelled or spelled differently in the 'German only' and the vocabulary sheet. Some of the English meanings also may be somewhat misleading. Pay no attention to any such irregularities which you may notice.

"Do not ever look at any vocabulary, German dictionary or the like except during the practice periods. Do not look at your test records at all after they are done. Put all such test records away safely where they cannot be lost or destroyed."

When in the first day or two of study it was found that some could learn 100 words in less than an hour, added instructions were given to the effect that the number of words to be studied in an hour could be raised to 150, 200, 300, 400 or 600 as seemed best. Consequently in the course of study individuals varied widely in the number of words studied in an hour. For example, the records of Bu. and of Hy. were as follows:

RECORD OF BU.

										Estimated from 20 per cent. of all written.
1st round.										
	1st study period.		60 min.		100 words.		100 correct.			
	2d	"	"	"	"	100	"	100	"	"
	3d	"	"	"	"	200	"	185	"	"
	4th	"	"	"	"	200	"	200	"	"
	5th	"	"	"	"	200	"	185	"	"
	6th	"	"	"	"	200	"	180	"	"
	7th	"	"	"	"	200	"	185	"	"
2d round.										
	8th	"	"	"	"	400	"	380	"	"
	9th	"	"	"	"	400	"	390	"	"
	10th and last		"	"	"	400	"	370	"	"

RECORD OF HY.

1st round.	Study periods 1–12.			60 min.		100 words studied.			41 aver. correct.			
2d	"	"	"	13–24	"	"	100	"	"	54	"	"
3d	"	"	"	25–36	"	"	100	"	"	73	"	"
4th	"	"	"	37-42	"	"	200	"	"	135	"	"

As a rule, when an individual felt reasonably confident that he knew practically all the 1,200 words, he was tested with the entire list. In the case of five of the twenty-two individuals this final test was taken after 33, 36, 42, 30 and 24 hours of

study, respectively, although they had by no means mastered the list. Two tests were given with the selected list of 120 words to all but two or three of the twenty-two at times such as to give for all, except a few of the very rapid memorizers, records of achievement from a smaller amount of study than was needed to master the entire 1,200. To test the permanence of the connections over a long interval another test with the selected list was given over a month after the end of all study. The arrangement of these four tests with respect to the amount of study antecedent to each and the interval between each and the *last* study period antecedent to each in the case of the first three, and between the test and the average date of the last round of study in the case of the fourth, is given for each individual in Table I., columns 12, 13, 16, 17, 20, 21, 24 and 25.

We have then for each individual a record like the following for J. A. R., showing by the score of the first round the ability fo learn and retain the words long enough to write them out immediately after the study period; showing by the score of the later rounds the saving in learning due to the retention of the effects of earlier rounds; showing by three tests the number of words learned well enough by a given amount of study to be held in mind for a few days; and showing by one test the number of words learned well enough by a given amount of study to be held in mind for a month or more.

In the preliminary test J. A. R. knew 96 of the 1,200 words; he then studied (Feb. 14-22) 100 words per hour for 12 hour-periods giving correctly at the ends of the hours 83, 85, 82, 80, 81, 85, 80, 88, 77, 74, 82, 77. His average for the first round was thus 81. In the second round he studied 100 words per hour for 12 hour-periods (Feb. 23–March 1) giving correctly 89, 90, 90, 93, 90, 88, 88, 94, 85, 94, 95, 88. His average for the second round was thus 90. Two hours after the close of the third study period of the second round, that is, after 15 hours of study, in a test with the selected list of 120 words, he gave correctly 40, corresponding to 400 out of the 1,200. In the third round (March 2–7) he studied 100 words per hour for 3 periods, scoring 100, 90 and 95 (estimated from 20 per cent. of the records) and then began a new round studying 200 words per hour, giving correctly at the ends of the hours 193, 189, 195, 183, 183, 185, or an average of 186. He did not take the second test with the list of 120 words. On March 16, 9 days after his last study period and 11 days after the average date of his last round of study, he was tested with the entire 1,200 words, and wrote correctly, as a result of 33 hours of study, 1,088. On April 15, 39 days after his last study period, he was tested with a list of 120 and wrote correctly 56, *i. e.*, 560 on the basis of the 1,200 list.

In order to measure the real achievements some discount must be made for the number of words known before any study. For instance, J. A. R.'s 1,088 words includes presumably all or nearly all of the 96 words he knew before any study. It would seem that 1,088 — 96 was the real achievement of his study. So simple a discount is not, however, infallible. For besides knowing well enough to write them these 96 words J. A. R. may have known other words well enough to relearn them more easily than if he had had no acquaintance whatever with them. On the other hand, the 96 scored as fully known, had in some cases to be learned over again because the translation given in the vocabulary was different from that which the individual had in mind.[1]

From a comparison of the results in the case of the nine who knew less than 100 of the 1,200 words in the preliminary test, with those for the six who knew over 170, it appears that to subtract the number of words known in the preliminary test from all achievement records is a sufficient allowance, that the remainders are conservative measures of the achievement due to the study periods of the experiment.

Thus for J. A. R., who knew 96 or 8 per cent. of the 1,200 words, the real achievement in the first round of study would average 81 — 8 or 73 new words learned in 60 minutes. His average achievement for the second round would be 90 — 8, or 82, new words, a gain of 9 over the first round. His average achievement in the test of 120 words after 15 hours' study would be 40 — 9 or 40 — 9.6 (according as we subtract those known of the 120 themselves or one tenth the number known of the 1,200). His average achievement in the final round with 200 words per hour would be 186 — 16, or 170, new words. As a result of 33 hours of study J. A. R. had learned 1,088 — 96, or 992, new words so as to write them a few days after the close of the study periods and 10 × (56 — 9), or 560 — 96 (according as we subtract the proportion known of the 120 themselves or the proportion

[1] Though the subjects of the experiment knew that in all scoring any correct translation would be scored as correct, they all learned the meanings given in the vocabulary, apparently finding it easier to do so than to take the time to assure themselves that their old understanding of the word was correct.

known of the entire 1,200), new words so as to write them 39 days after the close of the study periods.

All my general statements about memory achievements will be based on such discounted results. These are, I think, if anything more likely to underestimate than to overestimate the rapidity and permanence of memory for these paired associates.

Another matter of method must be noted. In all tests the entire record was utilized, but in the scores for the study periods, the estimate was made (1) from 20 per cent. of the vocabularies or (2) less often, from the entire record for omissions and from 20 per cent. of the vocabularies for errors, and but rarely from the entire record for both omissions and errors.

A third matter also needs mention. The results from the 1,200 list and the 120 list are comparable so far as concerns the proportion known in the preliminary test. The proportion was on the average the same within 1 per cent. But in the test of 1,200 words some help to memory was given by the grouping of tens in correspondence to the groups of tens studied. A word would be remembered in its ten which might not be remembered if seen in another connection. In the test with 120 words this help was of course lacking. It will not therefore be strictly fair to compare the results of the two sorts of tests. For instance, we cannot say of J. A. R. that of 992 words, known for a few days, exactly 470 were known for 40 days, the conditions of 'knowing' not being precisely the same. In point of fact, however, this difference is negligible, for in the case of 5 individuals who took the 1,200 test and the 120 test a few hours apart a few days after the last round of study, we find the results practically identical, namely:

A. Test with 120 (× 10).	*B.* Test with 1,200.	Difference (*A* − *B*).
1,140	1,164	− 24
1,110	1,097	+ 13
1,200	1,161	+ 39
1,080	1,135	− 55
1,170	1,189	− 19

Average difference − 9 or less than 1 per cent.

This result is corroborated by the fact that in three other cases tested with the two tests after the same amount of study but with the 120-word test after longer intervals (46, 26 and

100 hours compared with 19, 10 and 53 hours) the inferiority of the scores in the 120-word test averaged only 3 per cent.

The results of the experiment are given in the 25 columns of Table I., which states them more clearly and economically than is possible in a reasonably brief verbal description. In the text that follows I shall simply report certain main features of the results with respect to (1) the rapidity of formation of paired associates, (2) their permanence of retention, (3) the relation between an individual's capacity in the one respect and in the other, (4) the influence of practice upon the former capacity, and (5) the individual differences in respect to memory for paired associates.

TABLE I.

Individual.	Test with 1,200 Words before Any Study.				First Round of Study.			Second Round of Study.		
		Number of Meanings Correctly Given.	Percentage of Meanings Correctly Given.	Number of Meanings Correctly Given for 120-Word List.	Number of Words Studied per Hour.	Average Number Known at End of Hour.	Achievement per hour; *i.e.*, Column 5 Corrected by Columns 1 and 2.	Number of Days Between Average Dates of 1st and 2d Rounds.	Average Number Known at End of Hour.	Achievement per Hour.
	Sex.	1	2	3	4	5	6	7	8	9
Ab.	M	114	9.5	11	100	47	38	(7)	80	61
Ag.	M	330	27.5	26	100	96	69	(8)	184	129
Ba.	F	44	3.7	4	100	96	92	(5)	177	170
Bur.	M	328	27.3	30	200	186	131	(4)	380	271
But.	M	129	10.8	13	100	99	88	(12)	197	175
Cra.	F	324	27	41	200	198	144	(3)	397	289
Cri.	M	2	.2	1	100	23	23	(6)	53	53
D.	M	165	13.8	22	100	89	75	(6)[2]	187	129
E.	F	108	9	13	100	97	88	(5)	195	177
G.	M	92	7.7	13	100	94	86	(5)	189	174
Hi.	M	120	10	14	100	56	46	(14)	79	69
Hy.	F	—[1]	.4	—[1]	100	41	41	(6)	54	54
Ke.	F	4	.3	2	100	95	95	(6)	173	172
Ki.	M	30	2.5	3	100	84	82	(5)	167	162
J.	M	77	6.4	6	100	95	89	(8)	144	134
L.	M	135	11.3	14	100	97	86	(5)	184	161
J. R.	M	96	8	9	100	81	73	(7)	90	82
M. R.	M	124	10.3	8	100	63	53	(7)	78	68
Ro.	M	174	14.5	18	100	90	76	(9)	180	151
Ru.	F	79	6.6	8	200	190	177	(2)	294	274
S.	M	345	28.8	31	200	133	75	(4)	173	115
T.	M	197	16.4	15	100	94	78	(7)	175	142

[1] Not tested, but knew no German; probably not over 6 out of 1,200 had test been made.

[2] Approximate. Exact dates were not recorded.

TABLE I.—*Continued.*

Individual.	Test with 120 Words, February 25.				Test with 120 Words, March 6.			
	Number of Meanings Correctly Given.		Number of Study Hours Preceding.	Hours Since Close of Last Study Hour.	Number of Meanings Correctly Given.		Number of Study Hours Preceding.	Hours Since Close of Last Study Hour.
	Gross.	Corrected.			Gross.	Corrected.		
	10	11	12	13	14	15	16	17
Ab.	37	26	15	1.5	48	37	29	2
Ag.	65	39	12	1.5	100	74	21	18
Ba.	80	76	19	3	114	110	33	14
Bur.	75	45	10	60	111	81	10	184
But.	37	24	3	12	81	68	12	20
Cra.	117	77	17	38	120	79	17	268
Cri.	23	23	22	12	85	85	41	1
D.					63	41	16	0.5
E.	97	82	22	2	112	97	32	46
G.	91	78	21	1.5	112	99	29	26
Hi.					34	20	18	12
Hy.					27	27	36	14
Ke.	62	62	18	1.5	108	108	29	24+
Ki.	69	66	21	1.5	94	91	32	1+
J.	53	47	12	50	82	76	22	25
L.	90	76	18	13	117	103	29	19
J. R.	40	31	15	2				
M. R.	48	40	14	2				
Ro.	81	63	15	8	112	94	28	2
Ru.	98	90	16	144	100	92	16	360
S.	81	50	15	2	116	85	24	84
T.					90	75	21	1

THE RAPIDITY OF FORMATION OF PAIRED ASSOCIATIONS.

In the case of groups of 100 or 200 words to be retained for 15 or 20 minutes.

The number of meanings learned in an hour and retained long enough to write them out varied from 23 to 177 (see column 6 of Table I.) with the central tendency for the group at 80. The average deviation of the individuals from 80 is 23; the median deviation, 12.5. The 80 is too low; for if we take those who knew less than 10 per cent. of the words before studying them, the corresponding figures are: Central tendency, 86; average deviation, 26; median deviation, 9. Moreover, if the study hour had been spread over 150 words in the case of Ag., Ba., Bu., E., G., Ke., J., L., R. and T., higher records would probably have been obtained.

So far as I know, no experiments with learning in this way more than a small group of pairs have been made hitherto, nor has anyone ventured to predict what the rapidity of learning 80 or 90 such meanings would be. The general impression one gets from discussions of memory by psychologists and writers on education is that they estimate the capacity at less than half its real strength.

In the case of 1,000 to 1,100 words to be retained for three or four days.

Because of my unwillingness to impose any more inconvenience on the subjects of the experiment than was absolutely necessary, the number of hours of study and the exact date of

TABLE I.—*Continued.*

Individual.	Test with 1,200 Words. On the Average March 9.				Test with 120 Words, April 15.			
	Number of Meanings Correctly Given.		Number of Study Hours Preceding.	Hours Since Last Study Hour.	Number of Meanings Correctly Given.		Days Since Middle of Last Round of Study.	Per Cent. of Loss Since Test with 1,200 Words
	Gross.	Corrected.			Gross.	Corrected and Estimated for Entire 1,200 Words.		
	18	19	20	21	22	23	24	25
Ab.	427	313	33	24	41	296	40	7
Ag.	1,010	680	22	72	79	460	42	32
Ba.	1,164	1,120	33	12	62	576	42	47
Bur.	1,097	769	10	184	96	632	48	14
But.	1,175	1,046	18	24	94	811	28	23
Cra.	1,161	837	17	200	108	756	51	20
Cri.	1,099	1,097	47	16	70	698	39	37
D.	1,145	980	19	7	57	405	37	52
E.	1,190	1,082	32	19	76	652	45	43
G.	1,160	1,068	29	10	68	588	44	48
Hi.	937	817	36	16	52	400	33	54
Hy.	386	380	42	48	23	230	39	41
Ke.	1,135	1,131	29	24	86	856	42	24
Ki.	1,039	1,009	33	24	49	460	42	54
J.	1,126	1,049	26	16	79	713	38	31
L.	1,189	1,054	29	17	95	815	43	24
J. R.	1,088	992	33	216	56	464	41	52
M. R.	842	718	30	24	53	406	39	38
Ro.	1,175	1,001	31	7	75	576	40	44
Ru.	1,190	1,111	16	0	95	871	55	22
S.	1,160	825	24	6	104	695	44	10
T.	908	711	24	36	75	553	40	17

the test after the close of study vary widely so that the results do not lend themselves to a simple statistical presentation. An examination of the data of Table I., columns 19, 20 and 21, will, however, show that the central tendency of the group is to a score of approximately 1,030 new words known 18 hours after the last study period (and, as is shown by detailed records not given in the table, three days after the middle of the last round of practice), as a result of 30 hours of study. With the 30 hours of study there were approximately 8 hours of time spent during the tests made in writing out the words known at the close of each of the study periods. These were probably equivalent in value to about 5 hours of unconditioned study.

The variability of the group around this central tendency is such that half the cases would probably lie between 34 and 26 hours of study if all had been tested three or four days after the middle of the last round of study and had studied just long enough to make in such a test a record close to 1,030 new words. The range of the actual records is from: (Hy.) *380 words learned, in 42 hours of study, and retained to 48 hours after the last study period, or 4 days after the middle of the last round of study*, to (Bu.) *1,046 words, in 18 hours of study, retained 24 hours after last study period, or 5 days after the middle of the last round;* or to B., 769 words, *in 10 hours of study, retained still longer.* (The case of 1,111 words in 16 hours of study was in a test directly after the last study period.) Bu., that is, learned roughly three times as many words as Hy., in three eighths of the time taken by Hy. B. learns twice as many words as Hy. and in a fifth of the time.

The Permanence of Retention of Paired Associations.

Considering how quickly the connections are formed their persistence is remarkable. This is most fully shown by the results of a test made a month after all study had ceased. It is also shown: (1) By tests of certain of the subjects made several days after all study had ceased; (2) by the fact that in from two to five repetitions of study of the 1,200 words all but two of the twenty-two subjects could learn 90 per cent. or more of the

entire number; and (3) by direct comparisons of the achievements in the first round of study with the achievements in the second round, of achievements in the second round with those in the third round, and so on.

From the data of column 23 of Table I., considered in connection with the other relevant columns, I calculate that the central tendency of the group is a retention of 576 words to 37 days after the final 1,200-word test, 40 days after the last study period, and 42 days after the middle of the last round of practice, as a result of 27 hours of study plus the extra help gained from writing out the meanings in the tests, roughly equivalent to 6 hours of unconditioned study.

We may make as an estimate for the permanent result of 30 hours of study plus the necessary testing (the number of hours of study for which the central tendency of achievement, 1,030, in the final 1,200-word test, was given) 640 words, or, supposing the 1,200-word test not to have been given and the help it gave to later memory to have been absent, 620 words.

The group of 22 individuals being taken as the unit, we may say that 30 hours of study, plus 8 hours of testing, give command of 1,030 words for 3 days and of 620 of them for 42 days. The loss in the month and a third between the 3 days later and the 42 days later test is thus only 40 per cent.

The more significant measure — the central tendency of the losses when the losses are calculated for the 22 individuals separately — is only 35 per cent.

It would be possible in the cases of 17 of the subjects to estimate with some reliability the progress of loss from the time immediately at the end of the last study period. It would not be far amiss to say that the central tendency would be to a loss of a twentieth in an hour, a tenth in three days and between four and five tenths in 40 days. These per cents must be used intelligently in our inferences from them, since they are relative, in the case of such large groups of pairs, to the perfection with which the group was learned. For example, if 200 words, of which 195 were given correctly, say, on February 25, are studied again for an hour on February 28, and are thereafter tested, the loss in an hour will be very slight; whereas for 200 words of

which 120 were given correctly on February 28, studied an hour February 28, the loss an hour thereafter will be considerable. In the former case more words will be *over*-learned, that is, connected more firmly than is necessary for retention till barely the close of the hour.

The effect of such over-learning influences the results 3, or 40, or even 400 days later, too, of course. That a word is learned does not mean that it is learned to just the same extent as some other word learned by the same or another subject. I have, therefore, been careful to give figures of retention and loss always as of retention or loss *of certain results gained from stated amounts of study*.

A quantitative comparison with Ebbinghaus's classic experiment is not allowable because of the difference in the units of measurement of loss with lapse of time and the greater probability of *over*-learning in the present experiment. However, it is certain that the common inference from Ebbinghaus's figures that half the effect of memorizing is lost in less than an hour and two thirds in a day is very far from holding true of memory for paired associates.[1]

The Relation Between an Individual's Capacity with Respect to the Rapidity of Formation of Associations and His Capacity with Respect to Their Permanence.

Rapidity of formation is ambiguous, inasmuch as any connection, to be formed, as to be made permanent for some length of time. The particular relation which I have measured is that between (*A*) the amount learned per hour and remembered for 15 or 20 minutes in the first round of study, and (*B*) the proportion of that retained for three days which was retained for forty days. My measurement of this relationship is only approximate because the lack of uniformity in the conditions of study among the twenty-two subjects unfits the figures for ordinary statistical procedure. I have simply ranked the twenty-two in an order for their work in the first study round, and also

[1] This inference was found by Professor Cattell to be false also in the case of vocabularies learned as series; that is, so that both German and English words are given and the proper English word is given for each German word.

for the retention from the three-day to the forty-day test The rankings are given in Table II.

The correlation between *A* and *B*, by my calculation, is .4. The opinion that *those who* learn slowly remember long is evidently a superstition whose origin is probably the fact that *what* is learned thoroughly, and so by much study, is remembered well. The same person studying the same thing longer will naturally remember it longer! But it is the quick learners who are the good retainers.

TABLE II.

Individual.	Rapidity of Learning: Order in Achievement in First Round.	Permanence of Associations: Order in Amount of Retention (from Column 19 to Column 23 of Table I.).	Individual.	Rapidity of Learning: Order in Achievement in First Round.	Permanence of Associations: Order in Amount of Retention (from Column 19 to Column 23 of Table I.).
Ab.	21	1	Hy.	20	14
Ag.	17	11	Ke.	5.5	9
Ba.	5.5	17	Ki.	11	21
Bur.	3	3	J.	9	10
But.	5.5	7	L.	9	8
Cra.	2	5	J.R.	15.5	20
Cri.	22	12	M.R.	18	13
D.	14	19	Ro.	13	16
E.	5.5	15	Ru.	1	6
G.	9	18	S.	15.5	2
Hi.	19	22	T.	12	4

THE INFLUENCE OF PRACTICE UPON THE CAPACITY TO FORM PAIRED ASSOCIATIONS.

In the general capacity to form the associations well enough to retain them at the end of the hour there was no demonstrable improvement; that is, none in the course of the first round of twelve hours. No more words were learned in the last half of it than in the first. The facts are as follows:

Ab., G., Hi., Hy. and M. R., for whom the 100 words per hour were a sufficient test, averaged for the 12 successive periods (in order):

41 41 53 48 46 36 51 44 45 42 37 54

or, by twos,

41 50.5 41 47.5 43.5 45.5

or, by fours,

46 44 44.5

Du., Ki. and J. R., for whom the 100 words per hour were probably a sufficient test, averaged:

86 87 92 87 81 85 85 91 82 83 83 84

or, by twos,

86.5 89.5 83 88 82.5 83.5

or by fours,

88 85 83

For Ag., Bar., But., E., G., Ke., J., L., Ro. and T., for whom the test was too easy, our best estimate is the number of records 95 or over at each period. These are:

8 8 9 9 6 10 8 6 8 7 8 8

or, by twos,

16 18 16 14 15 16

Bur., Ru. and S., whose first rounds were irregular, showed no appreciable practice effect.

The same absence of improvement is observable within the second round.

The absence of practice effect in the case of this score of educated adults must not be assumed to be proof or even important evidence that in general the capacity to form and retain paired associations is not susceptible to improvement by training. With subjects who in their every-day life did not have already a vast amount of such training the result might well be different. It is significant that my subjects did not to any considerable extent vary their methods of study in the course of the experiment. They all came to use the method of 'recall,' that is, of covering up the English words (after studying them for a brief period) and trying to recall them at the sight of the German words, verifying their memories thereafter and refreshing their memories by the percepts only when necessary. And with very few exceptions they used this method from the start. One result of practice did appear. The work became easier in the sense of being more interesting, less objectionable.

The absence of practice effect in these subjects, does, however, differentiate memory for paired associations from memory over very short intervals for long series such as poems or short series such as nonsense lists, lists of objects and the like. For

in the latter cases a marked practice effect is found even with subjects of the age and training of these. I am inclined to think that the same relative difference would hold with untrained subjects, who would probably improve in learning vocabularies for moderate retention, but would improve still further in learning lists and passages for very brief retention.

The interpretation of this difference is not an aim of the present report, but it may be noted, that the hypothesis that the more a memory function involves grasping, rather than retaining, the more susceptible it will be to training, deserves experimental testing.

Individual Differences in Memory for Paired Associates.

The facts already given of the variability in memory achievements have shown the great individual differences in separate features of associative memory. I shall now compare the total records of certain individuals. Even within so homogeneous a group as a class of graduate students and seniors, and so small a group as 22 of these, we find such extremes as:

Hy., who could learn only 41 words in an hour so as to write them at its close;
who could learn only 270 words in 36 hours so as to write them 4 days later;
who could learn only 386 words in 42 hours so as to write them 4 days later;
who knew only 230 of the 386 words 35 days afterwards.

Ke., who could learn 95 words in an hour so as to write them at its close;
who could learn 1,131 words in 29 hours so as to write them 4 days later;
who knew 856 of the 1,131 words 37 days afterwards.

Hy. and Ke. are strictly comparable, as neither knew before study more than 6 words of the entire 1,200. Both worked under similar conditions. In view of the fact that Ke. would almost surely have done much better than 95 had she tried 150 or 200 rather than 100 words, we may put her superiority as:

3 to 1 in amount learned in an hour for short retention,
5 to 1 " " " " 30 hours for moderate retention,
4 to 1 " " " " " " " long retention,
5 to 4 in proportion retained from the moderate to the long interval.

Comparison is also fair between Hi. and Bu., who knew before any study 120 and 129 words, so that the method of discounting used can favor one or the other only by the barest trifle. The conditions of work were more unfavorable for Bu., so that the obtained differences should be, if anything, increased.

Hi. learned 46 per hour,
learned 79 per hour in the second round,
" 220 in 18 hours,
" 817 in 36 hours,
knew 400 words 33 days later.
Bu. learned 88 per hour,
" 197 per hour in the second round,
" 1,046 in 18 hours,
knew 911 words 28 days later.

Bu.'s superiority is:

2 to 1 in the amount learned in an hour for short retention,
5 to 1 " " " " " 18 hours for moderate retention,
4 to 1 " " " " " x " " long " [1]
8 to 5 in the proportion retained from the moderate to the long interval.

The record of Ab. is lower than that of Hi. and the record of Bur. is higher than that of Ke. or Bu., but the comparison of Ab. and Bur. is too involved.

[1] Estimated.